To: Fawcett, Allen[Fawcett.Allen@epa.gov]; Marten, Alex[Marten.Alex@epa.gov]
From: Kopits, Elizabeth
Sent: Mon 8/17/2015 4:08:13 PM
Subject: RE: Social Cost of Methane Comms
2015 08 14 comms_CB_QA_v2 (3) djm KG alm ks_EK.docx

Hi Allen and Alex,

What do you think of something like this? I mostly just rearranged things but can shorten/simplify further if need be.

Elizabeth

From: Kocchi, Suzanne
Sent: Monday, August 17, 2015 9:13 AM
To: Fawcett, Allen; Kopits, Elizabeth; Marten, Alex
Subject: FW: Social Cost of Methane Comms

FYI – I see you are listed but just in case you didn't get this (it took a while to show up in my inbox).

To: Lewis, Josh[Lewis.Josh@epa.gov]; Roberts, Martha[Roberts.Martha@epa.gov]; Marten, Alex[Marten.Alex@epa.gov]; McGartland, Al[McGartland.Al@epa.gov]
From: Kopits, Elizabeth
Sent: Mon 8/17/2015 1:49:32 PM
Subject: RE: SCC letter to EPW

Thanks!

From: Lewis, Josh
Sent: Monday, August 17, 2015 9:30 AM
To: Roberts, Martha; Kopits, Elizabeth; Marten, Alex; McGartland, Al
Subject: Fwd: SCC letter to EPW

Begin forwarded message:

From: "Lewis, Josh" <Lewis.Josh@epa.gov>
Date: August 12, 2015 at 10:59:43 AM EDT
To: "Niebling, William" <Niebling.William@epa.gov>, "Lubetsky, Jonathan" <Lubetsky.Jonathan@epa.gov>, "Friedman, Kristina" <Friedman.Kristina@epa.gov>
Cc: "Bailey, KevinJ" <Bailey.KevinJ@epa.gov>, "Haman, Patricia" <Haman.Patricia@epa.gov>
Subject: FW: SCC letter to EPW

Was able to track this down from Kyle. Was sent yesterday.

To: Marten, Alex[Marten.Alex@epa.gov]
From: Kopits, Elizabeth
Sent: Thur 7/30/2015 10:02:50 PM
Subject: Re: Social cost of carbon distribution

Ok sounds good

Sent from my iPhone

> On Jul 30, 2015, at 5:50 PM, "Marten, Alex" <Marten.Alex@epa.gov> wrote:
>
> not a problem. I liked your admin note and just recommended to Jenny that she use that. I will look at the media advisory first thing tomorrow. Non-Responsive
>
> --
> Alex Marten
> marten.alex@epa.gov

From: Kopits, Elizabeth
Importance: Normal
Subject: Accepted: Hold for NAS open session on SCC
Start Date/Time: Wed 9/2/2015 9:00:00 PM
End Date/Time: Wed 9/2/2015 10:00:00 PM

To: Marten, Alex[Marten.Alex@epa.gov]
From: Kopits, Elizabeth
Sent: Thur 7/30/2015 9:49:10 PM
Subject: Re: Social cost of carbon distribution

Thank you! I owe you one, or rather like 1000.
Let me know if you want to discuss the media advisory or just iterate by email. Apparently I am finally boarding soon. Airport total zoo

Sent from my iPhone

> On Jul 30, 2015, at 5:42 PM, "Marten, Alex" <Marten.Alex@epa.gov> wrote:
>
> Hi Akshay,
>
>
> Attached please find all of the underlying SCC draws from the simulations conducted, including for the 2010 emissions year. Please let us know if you have any further questions.
>
>
> --
> Alex Marten
> marten.alex@epa.gov
>
>
> ________________________________
> From: Kopits, Elizabeth
> Sent: Thursday, July 30, 2015 5:31 PM
> To: Akshay Ashok
> Cc: Frisch, Janet E; Wolverton, Ann; Gorman, Chad M; Marten, Alex
> Subject: Re: Social cost of carbon distribution
>
> Hi Akshay,
>
> Thanks for your inquiry. I am very sorry I meant to try to send you this earlier today but did not have a free minute, and now I am headed off on vacation. If it can wait for a couple of weeks let me know. Otherwise If I am able to access the files remotely then I will try to send them tomorrow. Alternatively I may ask my colleague Alex Marten (ccd above) to help you.
>
> Thanks,
> Elizabeth
>
> Sent from my iPhone
>
> On Jul 30, 2015, at 11:18 AM, "Akshay Ashok" <aashok@mit.edu<mailto:aashok@mit.edu>> wrote:
>
>
> Dear Janet,
>
>
>
> Thanks for forwarding the appropriate contacts.
>
>
>
> Elizabeth and Ann, I appreciate your assistance in this matter!

>
>
>
> Thanks,
>
> Akshay
>
>
>
> From: Frisch, Janet E [mailto:FrischJ@gao.gov]
> Sent: Tuesday, July 28, 2015 6:15 PM
> To: Akshay Ashok
> Cc: 'Kopits, Elizabeth'; 'wolverton.ann@epa.gov<mailto:wolverton.ann@epa.gov>'; Gorman, Chad M
> Subject: Social cost of carbon distribution
>
>
>
> Dear Akshay
>
>
>
> Elizabeth Kopits or Ann Wolverton with EPA could help answer your question. I have included them on this email.
>
>
>
> Kind regards,
>
>
>
> Janet
>
>
>
> Janet E. Frisch | Assistant Director
>
> US GOVERNMENT ACCOUNTABILITY OFFICE
>
> 701 5th Avenue, Suite 2700 | Seattle, WA 98104 | 206.287.4859
> Fax: 206.287.4872 | frischj@gao.gov<mailto:frischj@gao.gov> | www.gao.gov<http://www.gao.gov/>
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>
> --
>
>
>
> From: Akshay Ashok [mailto:aashok@mit.edu]
>
> Sent: Tuesday, July 28, 2015 1:54 PM
>
> To: Gomez, Jose (Alfredo)

>
> Subject: Social cost of carbon distribution
>
>
>
> Hi,
>
>
>
> My name is Akshay Ashok, and I am a PhD student at MIT working on the climate impacts of aircraft emissions. I am interested in the valuation of CO2 emissions impacts, and I came across a paper by the interagency working group on the social cost of carbon (http://www.whitehouse.gov/sites/default/files/omb/inforeg/scc-tsd-final-july-2015.pdf).
>
>
>
> I would like to obtain the distribution of SCC in 2010, but unfortunately the paper only contains summary statistics for SCC in 2020 (Figure 1 and table A5). Would you have the distribution for 2010 SCC, or could you direct me to the right person who might be able to?
>
>
>
> Thanks,
>
> Akshay
> <SCC 2013 TSD output - July 2015 revision.zip>

Cc: Marten, Alex[Marten.Alex@epa.gov]; Bowen, Jennifer[Bowen.Jennifer@epa.gov]
To: McGartland, Al[McGartland.Al@epa.gov]
From: Kopits, Elizabeth
Sent: Thur 7/30/2015 9:03:15 PM
Subject: Re: draft input for media advisory

This could use some work but here is what I have so far:

SCC:

The closed door launch meeting for the National Academies' project was held yesterday (July 30). The process is off to a smooth start and there continues to be strong coordination between EOP, EPA, and other agencies in this effort.

Public posting of general information about the project and committee membership by the Academies will occur by August 14th, or possibly a few days earlier. This will include a media advisory that will provide a 2 sentence description of the goals of the project, list the sponsors, and direct to the dedicated Academies' web page for this project. The first committee meeting (part of which will be open to the public) is scheduled for September 2-3.

Sent from my iPhone

On Jul 30, 2015, at 4:29 PM, "McGartland, Al" <McGartland.Al@epa.gov> wrote:

Can you draft a few sentences for the weekly report to the Administrator. I think I would stress the strong coordination with WH and other agencies, everything is on track, etc. And the media advisory should be prominent as well.

From: Kopits, Elizabeth
Sent: Thursday, July 30, 2015 3:29 PM
To: Marten, Alex; McGartland, Al
Subject: Re: draft input for media advisory

FYI- Kevin just called me asking about this morning's meeting so I gave him a full debrief over the phone. Seemed pleased and is on board with media advisory. Would be interested to see our suggested draft once the 3 of us are happy with it.

E.

Sent from my iPhone

On Jul 30, 2015, at 2:44 PM, "Kopits, Elizabeth" <Kopits.Elizabeth@epa.gov> wrote:

> Here's a start. Let me know what you think. I have to leave for the airport now but will continue to be available remotely.
>
> See you in a couple weeks! ☺
>
> <darft input for media advisory.docx>

To: Marten, Alex[Marten.Alex@epa.gov]; McGartland, Al[McGartland.Al@epa.gov]
From: Kopits, Elizabeth
Sent: Thur 7/30/2015 6:44:15 PM
Subject: draft input for media advisory
darft input for media advisory.docx

Here's a start. Let me know what you think. I have to leave for the airport now but will continue to be available remotely.

See you in a couple weeks! ☺

To: Rennert, Kevin[Rennert.Kevin@epa.gov]; Shouse, Kate[Shouse.Kate@epa.gov]; Marten, Alex[Marten.Alex@epa.gov]
Cc: McGartland, Al[McGartland.Al@epa.gov]; Bowen, Jennifer[Bowen.Jennifer@epa.gov]
From: Kopits, Elizabeth
Sent: Thur 7/30/2015 1:53:32 PM
Subject: RE: FYI - in case Oil&Gas is signed before Landfills (before I get back from vacation)

Hi Kevin,

I am compressed tomorrow and Ex 6 - Other but can be reached by email or phone if need be. I will try to monitor email for anything time sensitive and can work/join calls if absolutely necessary –e.g. Non-Responsive

Other fronts:

NAS SCC - I don't expect anything will be needed for the next couple of weeks. I can pull together whatever is needed for the first committee meeting when I get back.

Non-Responsive

Non-Responsive

Hope this helps.

Elizabeth

From: Rennert, Kevin
Sent: Thursday, July 30, 2015 9:36 AM
To: Kopits, Elizabeth; Shouse, Kate; Marten, Alex
Cc: McGartland, Al
Subject: RE: FYI - in case Oil&Gas is signed before Landfills (before I get back from vacation)

Non-Responsive

Thanks,

Kevin

From: Kopits, Elizabeth
Sent: Thursday, July 30, 2015 8:37 AM
To: Shouse, Kate; Marten, Alex
Cc: Rennert, Kevin; McGartland, Al
Subject: RE: FYI - in case Oil&Gas is signed before Landfills (before I get back from vacation)

Super, thanks!

From: Shouse, Kate
Sent: Thursday, July 30, 2015 8:35 AM
To: Kopits, Elizabeth; Marten, Alex
Cc: Rennert, Kevin; McGartland, Al
Subject: RE: FYI - in case Oil&Gas is signed before Landfills (before I get back from vacation)

Non-Responsive

Non-Responsive I'm planning to submit all of the same documents to both dockets. So, the SC-CH4 and SC-CO2 materials I sent to Hillary for landfills will also be sent to OAQPS for oil and gas.

From: Kopits, Elizabeth
Sent: Thursday, July 30, 2015 7:55 AM
To: Marten, Alex; Shouse, Kate
Cc: Rennert, Kevin; McGartland, Al
Subject: FYI - in case Oil&Gas is signed before Landfills (before I get back from vacation)

Hi All,

I don't have a good sense of the timing of these two rules, but, before I forget, I just wanted to mention that if O&G ends up being signed before Landfills, then the Q&A on the use of Marten et al. will have to be revised to reflect that – if anyone asks for it. Attached is the latest version (which you all already have) which reflect the use of Marten et al. in Landfills. I have highlighted the 3 instances where Landfills is mentioned; these would have to be changed to O&G, if that rule is signed first.

Also, Kate and Alex - do either of you know whether the full peer review document that Alex put together for Landfills has been uploaded to the O&G docket? If not, I think it needs to be added there too. I will still try to help you with any remaining responses to the O&G interagency comments if I can today. Non-Responsive

Thanks!

E.

To: Shouse, Kate[Shouse.Kate@epa.gov]
From: Kopits, Elizabeth
Sent: Wed 7/29/2015 3:21:21 PM
Subject: FW: SCC response letter
7.21.2015_JMI et al to EPA re SCC docs.pdf

FYI – In case you haven't see in it, here is the incoming letter pertaining to the email I just forwarded to you. From my conversation with Martha, it sounds like they typically include specifics regarding any document production in the transmittal email rather than the letter response I just sent you.

That's all I know for now.

Elizabeth

From: Roberts, Martha
Sent: Wednesday, July 29, 2015 10:38 AM
To: McGartland, Al; Kopits, Elizabeth
Cc: Lubetsky, Jonathan; Friedman, Kristina
Subject: RE: SCC response letter

Adding the incoming letter, as well as Jonathan and Kristina so we're all on the same email chain. Kristina, please circulate this draft response to the appropriate individuals in OAP.

Thanks all,

Martha

From: Roberts, Martha
Sent: Wednesday, July 29, 2015 10:32 AM
To: McGartland, Al; Kopits, Elizabeth
Subject: SCC response letter

Take a look at the draft response and let me know if you have any edits/comments. Feel free to circulate this to others in NCEE who should take a look. Thanks!

Martha

Martha Roberts

Counsel, Office of Policy

Environmental Protection Agency

Office: 202-564-2286

Cell: 202-380-6677

From: Kopits, Elizabeth
Location: TBD - Conference code Code Conference code
Importance: Normal
Subject: Declined: Tentative HOLD SCC Kevin Rennert Meeting
Start Date/Time: Mon 7/27/2015 7:30:00 PM
End Date/Time: Mon 7/27/2015 8:30:00 PM

Hi Natalie - Since Janet Means-Thomas already scheduled this same meeting for Wednesday (1pm) , can you please cancel this one for this afternoon?
Thanks!
Elizabeth

EPA TRACKING Number EPA-HQ-2013-009523 (1)

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August 23, 2013

National Freedom of Information Officer
U.S. Environmental Protection Agency
1200 Pennsylvania Avenue, NW (2822T)
Washington, DC 20460

Dear Sir or Madam:

This is a request made pursuant to the Freedom of Information Act ("FOIA"), 5 U.S.C. § 522 and the implementing regulations of the Environmental Protection Agency ("EPA"), for copies of the following records:

1) All records discussing, describing, referring to or interpreting the *Technical Support Document: -- Social Cost of Carbon for Regulatory Impact Analysis -- Under Executive Order 12866* (February 2010) ("*2010 TSD on SCC*") or the *Technical Support Document: -- Technical Update to the Social Cost of Carbon for Regulatory Impact Analysis Under Executive Order 12866* (May 2013) ("*2013 TSD on SCC*").

2) To the extent not provided in response to request #1, all records of information relied upon by the *Interagency Working Group on Social Cost of Carbon, United States Government* ("*IWG on SCC*") in the development of the *2010 TSD on SCC* and the *2013 TSD on SCC*, including but not limited to:

   a) Any presentations, reports, studies, computer or other analytical models, and data (including modeling data) created by or on behalf of the *IWG on SCC*, or provided to or obtained by the *IWG on SCC*.

   b) Any records that discuss, describe, interpret or otherwise relate to the Integrated Assessment Models ("IAMs") referenced in the *2010 TSD on SCC* or the *2013 TSD on SCC*, including but not limited to:

      i) any inputs and assumptions (including but not limited to assumptions on discounting, equilibrium climate sensitivity, and socioeconomic variables);

      ii) the results of any modeling runs or scenarios generated by the IAMs (in both paper copies and in original electronic form); and

      iii) the source codes for the DICE, PAGE and FUND models referred to in the *2010 TSD on SCC* or *2013 TSD on SCC* (in the original electronic form, such as native modeling formats).

c) Any records that identify, discuss, interpret or relate to any uncertainties or error rates of the *2010 TSD on SCC* or the *2013 TSD on SCC* or any of the IAMs referenced in or relied upon by the *2010 TSD on SCC* or the *2013 TSD on SCC*, including but not limited to any limitations in using the DICE, PAGE or FUND models to make predictions beyond the year 2100.

d) Any records that discuss, interpret or explain why the social cost of carbon estimated by the *2013 TSD on SCC* is approximately 60% higher than that estimated by the *2010 TSD on SCC*.

e) Any records that discuss, interpret, relate to, explain or make recommendations or decisions regarding global versus domestic measures of the social cost of carbon with respect to the work of the *IWG on SCC* and the *2010 TSD on SCC* or *2013 TSD on SCC*, including but not limited to the calculation or use of a domestic SCC for the United States.

3) Any records discussing, interpreting, transmitting or relating to any critiques, analyses or studies of the *2010 TSD on SCC* or *2013 TSD on SCC*, or any data, information or models relied on or referred to in the *2010 TSD on SCC* or *2013 TSD on SCC*, including but not limited to the work or statements of Robert Pindyck.

For purposes of this request, "records" mean documents, information, memoranda, letters, reports, drafts, communications, records of communications, telephone message records, calendars, agendas, meeting sign-in sheets, presentations, handwritten or typed notes, facsimile transmissions, electronic mail, transcripts or recordings (audio or visual) of meetings, tapes and all other types of records in the possession, control or custody of EPA or contractors working for EPA.

For all records responsive to these requests that are not produced based on an asserted exemption from disclosure, please prepare a privilege and/or exemption log describing, at a minimum: (i) the type of record withheld; (ii) the date(s) of the creation of the record; (iii) the subject of the record; (iv) the identity of the author and all recipients of the records; and (v) a detailed description of the basis upon which EPA is withholding the record (e.g., the claim of privilege, FOIA exemption, etc.). To the extent any responsive documents are withheld based upon a claim of privilege or other exemption from disclosure, please produce redacted copies of all non-privileged or non-exempt factual material contained within such documents.

As required by law, please provide the above records within twenty (20) business days of receipt of this FOIA Request. 5 U.S.C. § 552(a)(6)(A)(i). If it appears that it may not be possible to provide all of the records by the statutorily-mandated deadline, please provide the responsive records that are available as of the deadline (i.e., do not withhold responsive records past the

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deadline while searching for additional records), and please contact me promptly regarding any scheduling issues.

I confirm in advance my willingness to pay for all reasonable costs associated with searching for and copying these records. However, should these costs exceed $500, I ask that you contact me prior to proceeding.

Thank you for your prompt attention to this request. Please direct any inquiries, notices, or determinations to me at (713) 495-4508 or cbell@sidley.com.

Sincerely,

/s/ Christopher L. Bell

Christopher L. Bell

EPA Tracking Number EPA-HQ-2013-009618 (2)

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August 23, 2013

National Freedom of Information Officer
U.S. Environmental Protection Agency
1200 Pennsylvania Avenue, NW (2822T)
Washington, DC 20460

Dear Sir or Madam:

This is a request made pursuant to the Freedom of Information Act ("FOIA"), 5 U.S.C. § 522 and the implementing regulations of the Environmental Protection Agency ("EPA"), for copies of the following records:

1. All records identifying the dates and attendees of meetings, or dates of and participants in teleconferences or webinars involving, the *Interagency Working Group on Social Cost of Carbon, United States Government* ("*IWG on SCC*"), or any committee, working group, sub-committee, task force or project group thereof.

2. Any government contracts used to hire anyone to assist in the preparation of the *Technical Support Document: -- Social Cost of Carbon for Regulatory Impact Analysis -- Under Executive Order 12866* (February 2010) ("*2010 TSD on SCC*") or the *Technical Support Document: -- Technical Update to the Social Cost of Carbon for Regulatory Impact Analysis Under Executive Order 12866* (May 2013) ("*2013 TSD on SCC*"), including but not limited to, any government contracts used to hire anyone to perform the Integrated Assessment Modeling referenced in the *2010 TSD on SCC* or the *2013 TSD on SCC*.

3. Any records that describe, identify, contain, interpret or provide the dates or substance of briefings of political appointees in the Executive Branch on the *2010 TSD on SCC* or the *2013 TSD on SCC*, including but not limited to, PowerPoint presentations. In the response, please produce the requested briefing records in their native original electronic form (PowerPoint, Word, etc.).

4. All electronic mail, WebEx documents, Google Docs, text messages, instant messages, voice mails, facsimiles or any other records relating to the *2010 TSD on SCC* or *2013 TSD on SCC* that were sent to any person outside the *IWG on SCC*, including but not limited to documents sent to or received from: any of the authors of the publications listed in the References section of the *2010 TSD on SCC* or *2013 TSD on SCC*; any other academics; government contractors; consultants; Sierra Club, Natural Resources Defense Council or other

nongovernmental groups; and any other third-party whatsoever that was not part of the *IWG on SCC*.

5. All records discussing, describing, interpreting or relating to the applicability of or compliance with the Information Quality Act (P.L. 106-554, Section 515, 144 Stat. 2763 (2001)), or any Information Quality Act Guidelines published by the Office of Management and Budget, with respect to the work of the *IWG on SCC* or development, publication, use or dissemination of the *2010 TSD on SCC* or the *2013 TSD on SCC*.

6. All records discussing, interpreting or relating to the applicability of the Administrative Procedure Act (5 U.S.C. § 500 et seq.) to the *2010 TSD on SCC* or the *2013 TSD on SCC*, including but not limited to whether those documents should have been published for public review and comment.

For purposes of this request, "records" mean documents, information, memoranda, letters, reports, drafts, communications, records of communications, telephone message records, calendars, agendas, meeting sign-in sheets, presentations, handwritten or typed notes, facsimile transmissions, electronic mail, transcripts or recordings (audio or visual) of meetings, tapes and all other types of records in the possession, control or custody of EPA or contractors working for EPA.

For all records responsive to these requests that are not produced based on an asserted exemption from disclosure, please prepare a privilege and/or exemption log describing, at a minimum: (i) the type of record withheld; (ii) the date(s) of the creation of the record; (iii) the subject of the record; (iv) the identity of the author and all recipients of the records; and (v) a detailed description of the basis upon which EPA is withholding the record (e.g., the claim of privilege, FOIA exemption, etc.). To the extent any responsive documents are withheld based upon a claim of privilege or other exemption from disclosure, please produce redacted copies of all non-privileged or non-exempt factual material contained within such documents.

As required by law, please provide the above records within twenty (20) business days of receipt of this FOIA Request. 5 U.S.C. § 552(a)(6)(A)(i). If it appears that it may not be possible to provide all of the records by the statutorily-mandated deadline, please provide the responsive records that are available as of the deadline (i.e., do not withhold responsive records past the deadline while searching for additional records), and please contact me promptly regarding any scheduling issues.

I confirm in advance my willingness to pay for all reasonable costs associated with searching for and copying these records. However, should these costs exceed $500, I ask that you contact me prior to proceeding.

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Thank you for your prompt attention to this request. Please direct any inquiries, notices, or determinations to me at (713) 495-4508 or cbell@sidley.com.

Sincerely,

/s/ Christopher L. Bell

Christopher L. Bell

EPA Tracking Number EPA-HQ-2013-009617 (5)

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August 23, 2013

National Freedom of Information Officer
U.S. Environmental Protection Agency
1200 Pennsylvania Avenue, NW (2822T)
Washington, DC 20460

Dear Sir or Madam:

This is a request made pursuant to the Freedom of Information Act ("FOIA"), 5 U.S.C. § 522 and the implementing regulations of the Environmental Protection Agency ("EPA"), for copies of the following records:

1. All records discussing, describing, referring or providing direction to, communicating to or about, or interpreting the formation, organization, structure, authority, activities, meetings, recommendations or decisions of the *Interagency Working Group on Social Cost of Carbon, United States Government* ("*IWG on SCC*").
2. All records describing or discussing any past, on-going (as of the date of this submission) or planned work of the *IWG on SCC*.
3. All records describing, discussing or interpreting how the *Technical Support Document: -- Social Cost of Carbon for Regulatory Impact Analysis -- Under Executive Order 12866* (February 2010) ("*2010 TSD on SCC*") or the *Technical Support Document: -- Technical Update to the Social Cost of Carbon for Regulatory Impact Analysis Under Executive Order 12866* (May 2013) ("*2013 TSD on SCC*") have been or are to be used by Federal agencies, including but not limited to the use of these documents in the development and promulgation of regulations and in making policy decisions.
4. All records discussing or interpreting how the *2010 TSD on SCC* or the *2013 TSD on SCC* have been used or relied on, or are planned to be used, by EPA in any regulation proposed or promulgated by EPA, or in any policy decisions made by or guidance documents created by EPA.

For purposes of this request, "records" mean documents, information, memoranda, letters, reports, drafts, communications, records of communications, telephone message records, calendars, agendas, meeting sign-in sheets, presentations, handwritten or typed notes, facsimile transmissions, electronic mail, transcripts or recordings (audio or visual) of meetings, tapes and

EPA TRACKING NUMBER EPA-HQ-2013-005473

Pursuant to the Freedom of Information Act ("FOIA") 5 U.S.C. § 552, and the implementing regulations of the U.S. Environmental Protection Agency ("EPA"), 40 C.F.R. Part 2, I am requesting a copy of the following EPA records:

1. All petitions for rulemaking of any kind submitted to the EPA Administrator related to any statute or regulation under which the EPA Administrator has the authority to promulgate regulations from January 1, 2013, to the present. These include, but are not limited to, petitions under the Clean Air Act, the Clean Water Act, the Resource Conservation and Recovery Act, the Comprehensive Environmental Response, Compensation, and Liability Act, the Toxic Substances Control Act, and the Federal Insecticide, Fungicide, and Rodenticide Act.

2. All notices of intent to sue EPA or its Administrator, dated from January 1, 2013, to the present, under any of the statutes listed above or under the Administrative Procedure Act for:

a. Failure to timely act on any petition for rulemaking;

b. Failure to meet any statutory, regulatory, or other deadline; or

c. Failure to meet any nondiscretionary obligation..

This request is only seeking for the specific items referred to above, all of which would have been submitted to EPA by third parties. Therefore, this request does not seek any records that are exempt from disclosure.

I confirm in advance my willingness to pay for all reasonable costs associated with searching for and copying these records. However, should these costs exceed $250, I ask that you contact me prior to proceeding.

Please direct any inquiries, notices, or determinations to me at (202) 736-8281. Thank you for your anticipated assistance.

Description Available to the Public : Yes

Has Request Been Modified? ☐

Attached Supporting Files

No supporting files have been added.

A Guide to Economic and Policy Analysis of EPA's Transport Rule

Richard Schmalensee

Robert N. Stavins

March 2011

This White Paper was commissioned by the Exelon Corporation. It represents the views of the authors, not necessarily those of Exelon.

A Guide to Economic and Policy Analysis of EPA's Transport Rule

Richard Schmalensee and Robert N. Stavins[1]

March 2011

Executive Summary

The U.S. Environmental Protection Agency (EPA) is developing new rules to regulate the interstate transport of sulfur dioxide (SO_2) and nitrogen oxides (NO_X) emitted from electric power generation facilities. EPA's regulatory proposal – the Clean Air Transport Rule (Transport Rule) – is designed to help communities that are generally downwind of major emissions sources comply with air quality standards and, in the process, provide health and environmental benefits to upwind and downwind communities alike. The Transport Rule is one in a series of rules being developed by EPA that will affect the electric power sector, including regulation of GHG emissions, hazardous air pollutants, cooling water intake structures, and waste disposal for coal combustion bi-products.[2]

As EPA undertakes this series of rulemakings, we believe the public interest requires the Agency to carefully assess decisions about the stringency, design, and timing of proposed rules in a holistic framework that appropriately accounts for the regulation's likely effects. This framework is grounded in "benefit-cost analysis," a key element of regulatory impact assessments required through Executive Orders spanning the past five Presidential administrations, and is complemented by distributional assessment of the economic impacts to regions, sectors and populations. Using this lens, several important points about the Transport Rule emerge:

- **Existing studies providing estimates of the Transport Rule's benefits and costs consistently find that benefits outweigh costs on a national basis, often by a wide margin.**

  EPA estimates that benefits of the Transport Rule are 25 to 130 times greater than the corresponding estimated costs. The benefits come in many forms, with the largest coming

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[2] EPA is also reconsidering recently revised National Ambient Air Quality Standards (NAAQS) for ozone that would likely require additional seasonal NO_x reductions beyond those required by the Transport Rule. EPA is also expected to propose new $PM_{2.5}$ NAAQS later in 2011 that would potentially require additional annual SO_2 and NO_X reductions.

from reduced premature mortality. Reduced morbidity, especially lower incidence of respiratory and heart disease, improved visibility, enhanced agricultural and forestry yields, environmental amenities, and improved ecosystem services would also be achieved. The billions of dollars in savings expected from reduced health care expenditures and improved worker productivity alone may more than offset the Transport Rule's compliance costs.

- **The net positive benefits of the Transport Rule estimated by these studies are robust to changes in certain key modeling assumptions.**

  Differences in key assumptions used to estimate the benefits of reduced premature mortality largely drive the wide variation in estimated benefits of the Transport Rule, which accounts for the vast majority (80 to 96 percent) of total estimated benefits. These studies' findings of positive net benefits continue to hold with changes to several key modeling assumptions.

- **Actions taken to achieve regulatory benefits also create social costs. The proposed timing of the Transport Rule's requirements appear unlikely, however, to raise the national costs of implementation significantly.**

  Given the anticipated quantity of required pollution control retrofits, and the limited quantity of coal-fired capacity expected to retire under Transport Rule, and excess capacity in many regions, we should be able to easily avoid substantial transition costs.

- **Expanded supplies of low-cost natural gas and currently underutilized labor supply to help install pollution control equipment may well lower the social cost of the Transport Rule and mitigate any impact on electric rates.**

  Although coal prices have risen over the past decade, technological advances in natural gas extraction have greatly expanded economically viable supplies of unconventional sources. By making natural gas-fired generation more competitive with coal-fired generation, these price trends can not only lower the cost of reducing emissions through fuel substitution, but also contribute independently to coal-fired plant retirement decisions along with other market factors, such as EPA regulation.

  Similarly, in difficult economic times, such as today's, when unemployment is high, some workers used to meet new regulatory requirements may otherwise have been unemployed or underemployed. Thus, using their labor to implement the regulation imposes a lower social cost. Consequently, implementing the Transport Rule during periods of high unemployment may lower the Rule's social costs.

- **"Upwind" states, in addition to "downwind" states, will receive substantial benefits from the Transport Rule.**

  Although designed to address "upwind" states' power plant pollution impacts on "downwind" states, this characterization may misrepresent the geographic distribution of the Transport Rule's benefits and costs. While the Rule's economic costs most likely will be borne in upwind states relying heavily on coal-fired power, because of reduced emissions, these states also would likely receive substantial benefits from the Rule, largely in the form of improved health outcomes. The benefits will include reductions in health care expenditures and improved worker productivity, as well as improvements in well-being.

- **Employment will likely rise in the short run as a consequence of the Transport Rule, due largely to investment in new pollution controls.**

  In the short run, the installation and operation of new pollution control equipment and construction of new generation to replace retired coal-fired generation under the Transport Rule are likely to outweigh any reduced employment at retiring coal-fired facilities. In the long run, given the many adjustments within and outside the electric sector, the Transport Rule's impact on net employment could be positive or negative.

In sum, while imposing incremental costs to achieve reductions in SO_2 and NO_X emissions, the Transport Rule would produce significant benefits in terms of improved health outcomes, and better environmental amenities and services, which studies estimate significantly outweigh the costs.

I. EPA's Transport Rule and Other Rulemakings Affecting the Electric Power Sector

The Transport Rule is being developed to satisfy the "good neighbor" provision of the Clean Air Act (CAA), which requires that upwind states reduce emissions that "contribute significantly" to downwind states' nonattainment with (or maintenance of) National Ambient Air Quality Standards (NAAQS) for two key pollutants: ozone and fine particulate matter ($PM_{2.5}$).[3] Because SO_2 and NO_X are "precursors" to ozone (i.e., smog) and $PM_{2.5}$,[4] reductions in upwind SO_2 and NO_X emissions can help reduce ambient ozone and $PM_{2.5}$ concentrations in downwind regions.[5]

The Transport Rule is EPA's second effort to satisfy these "good neighbor" provisions. Because of "legal flaws", the U.S. Court of Appeals (the Court) remanded EPA's first effort, the Clean Air Interstate Rule (CAIR), but required that CAIR remain in effect until a rule addressing the Court's concerns was promulgated. Under the Transport Rule, power plants will need to comply with Phase I state-specific emissions targets in 2012 and more stringent Phase II emission targets in 2014. As shown in Table 1 below, these targets represent reductions of between 30 to 60 percent below anticipated (Baseline) emissions in 2012 absent the Transport Rule. Notably, however, because of industry over-compliance with existing SO_2 and NO_X emission requirements,[6] 2009 emissions already were substantially below the Baseline level. Indeed, NO_X emissions in Transport Rule states in 2009 were below aggregate emissions under both 2012 and 2014 Transport Rule requirements, whereas achieving aggregate SO_2 2014 targets will require about a 40 percent reduction relative to 2009 emissions.[7]

In its ruling, the Court invalidated the core of prior SO_2 and NO_X regulation, a cap-and-trade system with unlimited trading across states, which had afforded maximum compliance flexibility.[8] While fixing the spatial distribution of emissions may provide greater assurance that upwind emission reductions will help downwind regions achieve and maintain NAAQS compliance, it also limits the opportunity to shift emission reduction efforts to locations where they are least costly. Therefore, the proposed Transport Rule has the potential to raise the costs of

[3] US EPA, 2010. "Federal Implementation Plans To Reduce Interstate Transport of Fine Particulate Matter and Ozone, Proposed Rule." *Federal Register* 75(147):45210-45465. CAA Section 110(a)(2)(D). EPA has previously promulgated rules to satisfy the "good neighbor" provision, including the NO_X SIP Call in 1998, which reduced NO_X emissions to assist downwind states' compliance with the ozone NAAQS.

[4] Through chemical reactions in the atmosphere, both SO_2 and NO_X emissions can lead to atmospheric ozone and fine particulates, both of which have adverse health consequences. ($PM_{2.5}$ refers to fine particulates smaller than 2.5 micrometers which can be inhaled deeply causing serious respiratory problems.) Ozone, commonly known as smog, is formed in the atmosphere when hydrocarbon vapors react with nitrogen oxides in the presence of sunlight. Both SO_2 and NO_X can be transformed through atmospheric chemical reactions into small particulates.

[5] The Transport Rule would limit annual SO_2 and NO_x emissions in 28 states, and seasonal NO_x emissions in 26 states.

[6] SO_2 emissions are currently capped at 8.95 million tons in 2010 annually under Title IV of the 1990 CAA. Many factors have contributed to over-compliance, including the banking of allowances to comply with future requirements.

[7] Because the Transport Rule will require compliance with individual state budgets, the reductions necessary for each state to meet its state budgets will vary.

[8] Prior regulations include the Title IV SO_2 Trading Program, the Ozone Transport Commission NO_X Budget Program, and the NO_X SIP Call.

achieving aggregate reductions in SO_2 and NO_X emissions.[9] Notably, however, EPA's preferred design for the proposed rule establishes state-specific emissions caps or budgets for power plant SO_2 and NO_X emissions, and allows intra-state emissions trading and limited inter-state emissions trading.[10] As such, to help lower compliance costs, EPA's proposal provides some compliance flexibility while also addressing the Court's concerns regarding CAIR.[11]

Table 1
SO_2 and NO_X Emissions: Actual and Projected Compared to Transport Rule Targets For Transport Rule States (Million Tons)

			2012		2014	
	2008	2009	Baseline	Transport Rule	Baseline	Transport Rule
SO_2	8.9	4.7	8.5	3.6	7.4	2.8
percent reduction relative to 2009				23%		40%
percent reduction relative to baseline				58%		62%
NO_X (annual)	2.2	1.3	2.2	1.4	2.1	1.4
percent reduction relative to 2009				-6%		-6%
percent reduction relative to baseline				36%		33%
NO_X (summer)			0.7	0.6	0.7	0.6

Note: Baseline emissions are EPA's estimate of future emissions assuming pre-existing regulatory requirements (e.g., Title IV SO_2 trading) and economic factors affecting the operation of pollution control equipment.
Source: EPA, June 2010; EPA, Acid Rain Program Progress Reports, for 2008 and 2009.

Given the potential for flexibility to lower compliance costs, as it develops other power sector rules, EPA should endeavor to continue to allow as much compliance flexibility as is feasible. Flexibility can emerge in various ways in the different EPA rule-makings. For example, future regulations of cooling water intake structures, designed to reduce fish impingement and entrainment, could provide flexibility by allowing consideration of site-specific circumstances, and by including the potential for restoration at other locations to offset impacts from the intake structure.[12]

[9] The Court's decision on CAIR has effectively ended the nationwide SO_2 allowance trading system created by Title IV of the 1990 CAA. That system embodied the assumption that all SO_2 emissions are environmentally equivalent, regardless of their location, thus achieving some reduction in compliance costs at the expense of a more certain distribution of benefits.

[10] The Transport Rule proposes state-specific caps, based on each state's contribution to downwind nonattainment, in response to the Court's concern that CAIR's cap-and-trade program did not sufficiently assure elimination of upwind sources' "significant contribution" to downwind nonattainment.

[11] EPA is also considering two alternatives to its preferred design. Both options limit trading flexibility and thus would raise the costs. One alternative prohibits inter-state trading entirely, while allowing intra-state trading. The other "direct control" option would cap state-level emissions, impose emission rate standards on all facilities, and allow averaging across each company's facilities within each state.

[12] In some respects, EPA's proposed rule for cooling water intake structures embodies such flexibility. EPA, Proposed Regulations to Establish Requirements for Cooling Water Intake Structures at Existing Facilities and Amend Requirements at Phase I Facilities, March 28, 2011.

II. Fundamentals of Economic and Policy Evaluation of Environmental Regulations

Economic analysis of proposed regulations is a critical element of the regulatory process. When performed properly, such analysis can contribute valuable information for constructive public deliberations on new policies and can help ensure that regulations provide positive net benefits to society.[13] Benefit-cost analysis is the core element of a sound economic assessment of a proposed regulation. Within the benefit-cost framework, the full benefits and costs of proposed policies are estimated and aggregated to determine which regulatory approach (including the option not to regulate) is likely to provide the greatest net benefits (benefits minus costs).[14] When benefits and/or costs occur over time, as they typically do, discounting is performed to aggregate over different time periods. To the extent possible, both benefits and costs are estimated in monetary terms, to enable direct comparisons. Applying benefit-cost methods to alternative policies – including different stringency levels, implementation schedules, and/or policy instruments – can help identify which alternative provides the greatest net benefits to society.

Environmental regulations can provide a range of benefits associated with human health (including enhanced well-being, lower health expenditures, and increased worker productivity), greater public and workspace safety, better recreational experiences, improved visibility, enhanced aesthetic amenities, and improved ecological services. Due to the lack of markets for many of these benefits, monetizing their values raises many challenges. However, available empirical methods can reliably determine individuals' willingness-to-pay for improvements in health, recreational experiences and environmental conditions, while other methods can provide proxies for benefits when such estimates are not available.

Actions taken to achieve regulatory benefits also create social costs.[15] While the availability of relevant markets simplifies certain aspects of cost analysis, accurately capturing the economic impacts of new rules, particularly under uncertain future conditions, raises many challenges. These impacts reflect actions taken to comply with the regulation (compliance costs), plant shutdowns, job losses, and production disruptions arising in the transition to new regulations (transition costs), and the impact of higher prices on the broader economy (general equilibrium costs).

Assessment of a regulation's distributional impacts complements benefit-cost analysis. Even though a regulation creates net gains for society as a whole, it may nonetheless make some groups worse off. Distributional assessments focus on whether certain industries, income groups, or geographic regions are likely to experience particularly positive or negative net impacts from the proposed regulation. Such analysis can provide policymakers with an opportunity to modify the regulation or supplement it with additional measures to address these distributional impacts.

Given the many benefits of comprehensive regulatory impact assessments, administrations dating back to the Reagan era have required such assessments for all proposed "major" federal

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[13] *See*, Arrow, Kenneth, Maureen Cropper, George Eads, Robert Hahn, Lester Lave, Roger Noll, Paul Portney, Milton Russell, Richard Schmalensee, Kerry Smith, and Robert Stavins. "Is There a Role for Benefit-Cost Analysis in Environmental, Health, and Safety Regulation?" *Science*, April 12, 1996.

[14] Even when statutory requirements limit agency discretion to design regulations to maximize net benefits (such as the setting of NAAQS), agencies are still required to analyze benefits and costs.

[15] For a more complete taxonomy of regulatory costs, see: Jaffe, Adam B., Steven R. Peterson, Paul R. Portney, and Robert N. Stavins. "Environmental Regulation and the Competitiveness of U.S. Manufacturing: What Does the Evidence Tells Us?" *Journal of Economic Literature* 33(1995):132-163

regulations.[16] To this end, the White House Office of Management and Budget (OMB)[17] and EPA itself[18] developed guidance documents to define more formally the scope and methods that analysts should use to create rigorous, balanced, and ultimately informative analyses.

III. Analyzing the Benefits and Costs of EPA's Transport Rule

The Transport Rule would lead to both new benefits – incremental to those achieved by existing Federal and state emission requirements – and new costs, as the electricity sector takes additional steps to meet stricter limits on power plant emissions. However, studies estimating the Transport Rule's benefits and costs have consistently found that benefits outweigh costs, on a national basis, often by a wide margin. In this section we enumerate some of the benefits and costs that would be created under the Transport Rule and discuss important issues for their proper assessment.

A. Benefits

Existing EPA regulations to limit emissions of SO_2, NO_X and other criteria pollutants have created significant benefits in terms of health improvements, aesthetic amenities, recreational benefits, and ecosystem enhancements. OMB estimates that EPA air rules in place as of 2010 account for \$93 billion to \$629 billion (2009\$)[19] in annual benefits, reflecting the vast majority (94 to 97 percent) of the benefits from all EPA regulations and a large share (60 to 84 percent) of the benefits from all federal regulation.[20] Most of these air quality benefits are attributable to rules that target reductions in $PM_{2.5}$ pre-cursor emissions of SO_2 and NO_X. EPA estimates even larger annual benefits – \$1.3 trillion annually in 2010 – from the CAA than those estimated by OMB.[21]

The electric power sector currently accounts for roughly 75 percent of national SO_2 emissions and 20 percent of national NO_X emissions.[22] Further reductions in SO_2 and NO_X power

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[16] E.O. 12866, signed by President Clinton in 1993, outlines the rationale, goals and requirements of federal regulatory review. This was preceded by related Executive Orders, notably E.O. 12291, signed by President Reagan in 1982. E.O. 12866 has been subsequently amended, but its primary provisions remain intact. The Obama administration recently issues E.O. 13563, which "adds and amplifies" to E.O. 12866.

[17] Office of Management and Budget, Circular A-4, September 17, 2003. OMB Circular A-4 outlines "best practices" that agencies should use in conducting regulatory analyses.

[18] EPA's Guidelines were revised and re-released in 2000. Revisions were made in collaboration with outside experts and its Science Advisory Board. One of the authors chaired the SAB's Environmental Economics Advisory Committee at that time. EPA, "Guidelines for Preparing Economic Analyses," EPA 240-R-00-03, September 2000.

[19] Throughout the paper, values from other studies are converted into 2009 dollar values using the GDP price deflator. Bureau of Economic Analysis, "Price Indexes for Gross Domestic Product," 2010.

[20] These aggregate figures generally reflect benefits as estimated by EPA. OMB, Office of Information and Regulatory Affairs. "2010 Report to Congress on the Benefits and Costs of Federal Regulations and Unfunded Mandates on State, Local, and Tribal Entities." 2010, pp. 10-14.

[21] This estimate includes reductions from CAIR and the Clean Air Mercury Rule, which has since been vacated. "The Benefits and Costs of the Clean Air Act from 1990 to 2020," Office or Air and Radiation, March 2011.

[22] EPA, Nitrogen Oxide and Sulfur Dioxide Emissions by Sector, 2005.

plant emissions can potentially yield a wide variety of benefits, including reduced mortality, reduced incidence of respiratory and heart disease, improved visibility, enhanced agricultural and forestry yields, greater environmental amenities, and improved ecosystem services.[23] Moreover, these benefits come in many forms, including improved well-being, reduced health-care expenditures, and improved work-productivity from reduced sick days. The magnitude of these health benefits will depend upon the size and location of emission reductions, the resulting improvements in air quality, and the valuation of health benefits that arise from these air quality improvements.

As shown in Table 2, estimates of the benefits from further reducing SO_2 and NO_X power plant emissions vary widely across studies. [See end of draft – will be embedded in text in final draft.] Estimates of total benefits range from a low of $20 billion annually to a high of $310 billion annually. The sizable variation in estimated total benefits is driven largely by differences in key assumptions used in estimating the benefits of reduced premature mortality, which accounts for the vast majority (80 to 96 percent) of total estimated benefits.[24] These key assumptions include air transport and health effects modeling, and the value of a statistical life (VSL), which measures the benefits of reduced mortality risk. We examine the sensitivity of total benefits to these key assumptions by holding constant assumptions related to either VSL or health effects across studies. As shown in column [f] of Table 2, VSL estimates used in recent studies range from $2.8 million to $8.3 million.[25] Table 2, column [g] shows that using a $7.3 million VSL, as recommended in EPA's most recent economic guidelines and endorsed by EPA's Science Advisory Board's Environmental Economics Advisory Committee, substantially narrows the range of total benefits across studies, producing a minimum value of $48 billion in benefits.[26]

[23] For example, reductions in nitrogen and acid deposition may improve agricultural and forestry yields.

[24] Our assessment of Transport Rule also considers studies of CAIR, given certain similarity in the policies' requirements. Differences in the quantity and geographic distribution of emission reductions from each policy will likely lead to differences in estimated and actual benefits and costs.

[25] VSL methods typically rely upon either differences in wages between more and less risky jobs or survey methods to determine people's willingness to pay for reductions in mortality risk. Even in economic terms, VSL is not intended to capture the "value of a life." Rather, VSL reflects the aggregate value that a large number of individuals would be willing to pay in exchange for a small reduction in mortality risk.

[26] One of the authors was chair of the Committee at the time of this review.

Table 2
Summary of the Economic Benefits of Reduced Electric Power SO_2 & NO_x Emissions from Various Studies

Source	Year/ Case	Policy [a]	Benefits [b]	Mortality Benefits [c]	Costs [d]	Net Benefits [e]	VSL [f]	Total Benefits with Fixed Values for: VSL: EPA SAB Value [g]	Health Effects: Pope et al. Effects [h]	Health Effects: Palmer et al. Effects [i]
EPA [1]	2014 / 3% + Pope	Transport Rule	$128.9	$116.8	$2.4 $4.3	$126.6 $124.6	$8.3	$114.9	$118.3	$75.0
	2014 / 3% + Laden	Transport Rule	$309.4	$297.2	$2.4 $4.3	$307.0 $305.0	$8.3	$273.7	$118.3	$75.0
	2014 / 7% + Pope	Transport Rule	$118.3	$106.2	$2.4 $4.3	$115.9 $114.0	$8.3	$105.6	$118.3	$75.0
	2014 / 7% + Laden	Transport Rule	$277.5	$265.4	$2.4 $4.3	$275.2 $273.2	$8.3	$245.7	$118.3	$75.0
EPA [2]	2010, 3% discount	CAIR	$92.6	$85.0	$2.4	$90.2	$7.6	$89.3	$87.5	$54.9
	2010, 7% discount	CAIR	$79.1	$71.5	$2.7	$76.4	$7.6	$76.3	$87.5	$54.9
	2015, 3% discount	CAIR	$127.6	$117.2	$3.2	$124.4	$8.1	$116.0	$103.2	$65.4
	2015, 7% discount	CAIR	$109.0	$98.7	$3.9	$105.2	$8.1	$99.3	$103.2	$65.4
Burtraw et al. [3]	2010	CAIR (with Mercury CAP)	$20.6	$17.2	$3.7	$17.0	$2.8	$47.6	$32.5	$20.6
	2020	CAIR (with Mercury CAP)	$29.5	$23.7	$7.1	$22.4	$2.8	$66.6	$44.3	$28.6
National Research Council [4]	2005	Full Damages	$63.9	$60.1			$6.2	$74.6		

Adjusted benefits estimates are calculated as follows:

- EPA SAB VSL – Assumes a value of statistical life of $7.3 million in $2009, based upon the EPA's Guidance for Economic Analysis, which has been reviewed by the EPA's Science Advisory Board Environmental Economics Advisory Committee.
- Pope et al. Health Effects – Assumes levels of avoided premature mortality from reduced SO_2 and NO_X emissions as estimated by EPA and based upon health effect coefficients from Pope et al. (2002) for PM and Bell et al. (2004) for ozone, assuming a 7 percent discount rate when discounting future mortality reductions.
- Burtraw et al. Health Effects – Assumes levels of avoided premature mortality from reduced SO_2 and NO_X emissions as estimated by Burtraw et al for benefits in 2010. These estimates were the lowest health effects among studies analyzing SO_2 and NO_X emissions reductions from eastern states. The NRC study had lower health effects, although this study analyzed national emissions, including emissions from western states that are targeted by the Transport Rule.

Estimates using fixed values for health affects assume an allocation of health effects to SO_2 and NO_X emissions based on estimates from the National Research Council study.

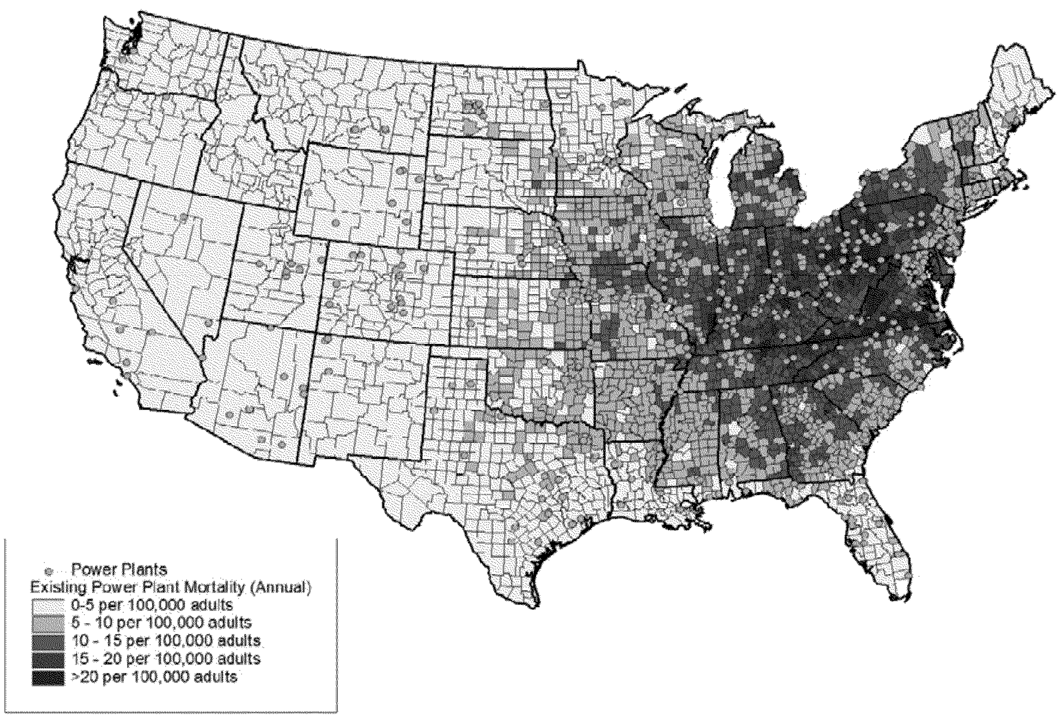
Table 2 (Continued)
Summary of the Economic Benefits of Reduced Electric Power SO_2 & NO_x Emissions from Various Studies

Notes:
All figures in 2009 dollars.
[1] Estimated benefits reflect health and environmental improvements in the eastern United States regardless of whether a community is in compliance with health-based NAAQS standards for ozone and $PM_{2.5}$, Estimates from the National Research Council Reflect benefits from the elimination of *all* emissions from electric power generation.
[2] EPA provides two measures of social cost for Transport Rule. The first measure (reported first) reflects (Hicksian equivalent) economic surplus over future years. The second measure (reported second) reflects direct compliance costs, including the annual cost of CAIR-related capital investment. Palmer et al.'s cost estimate reflects the net change in producer and consumer surplus. Estimates of incremental direct costs (including control and fuel) are $3.0 in 2010 and $6.8 billion in 2020.
[3] Burtraw et al. analyze the costs associated with CAIR, targeting SO_2 and NO_X emissions, and a cap on national mercury emissions. However, when estimating benefits, they only consider benefits in reduced $PM_{2.5}$ and ozone associated with CAIR emission reductions.

Sources:
[1] EPA, "Regulatory Impact Analysis for the Final Clean Air Interstate Rule," EPA-452/R-05-002, March 2005.
[2] EPA, "Regulatory Impact Analysis for the Proposed Federal Transport Rule," Docket ID No. EPA-HQ-OAR-2009-0491, June 2010.
[3] Palmer et al., 2005, "Reducing Emissions from the Electricity Sector, The Costs and Benefits Nationwide and for the Empire State," Resources for the Future Discussion paper 05-23, June; Palmer et al., 2007, "The benefits and costs of reducing emissions from the electricity sector," *Journal of Environmental Management*, 83(2007): 115-130.
[4] National Research Council, 2009, *Hidden Costs of Energy: Unpriced Consequences of Energy Production and Use*, Washington, D.C.

Each study's estimated mortality benefits also reflect the complex series of modeling steps needed to translate reductions in upwind SO_2 and NO_X emissions first into changes in ambient ozone and $PM_{2.5}$ concentrations, and then into changes in health outcomes. To examine the sensitivity to these modeling assumptions, we assume health effect values (reduced mortality per ton of emissions reduced) from EPA's most conservative Transport Rule scenario.[27] As reported in Table 2, column [h], when these health effects are used, estimated benefits range from $33 to $118 billion. When more conservative health effects are used, as shown in Table 2, column [i], estimated benefits range from $21 to $75 billion.[28] Together, these results suggest that estimated mortality benefits appear fairly robust to reasonable alternative values for these key assumptions.

Figure 1
Mortality Rates from Small Particulates from Coal-fired Power Facilities



Source: Clean Air Task Force, 2010. Analysis by Abt Associates.

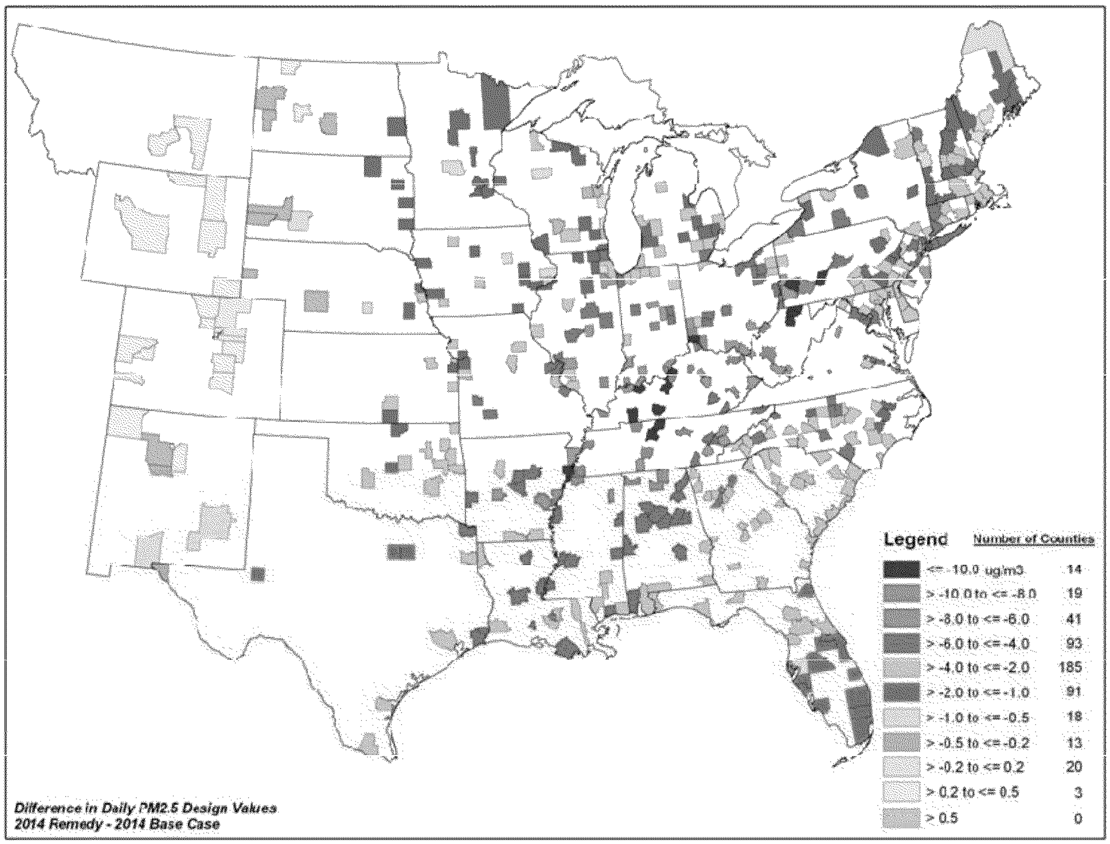
While designed to address downwind non-attainment, the Transport Rule also provides significant benefits to the upwind regions that reduce emissions. As illustrated in Figure 1, premature mortality from coal-fired power plants is most significant in the mid-western and eastern

[27] This EPA scenario assumes mortality (health) effect coefficients from Pope et al. for PM and from Bell et al. for ozone, and a 7 percent discount rate when valuing future mortality reductions. Bell, M.L. et al., 2004, "Ozone and short-term mortality in 95 US urban communities, 1987-2000," *Journal of American Medical Association*, 292(19): 2372-9; Pope, C.A., 2011, "Lung Cancer, Cardiopulmonary Mortality, and Long-term Exposure to Fine Particulate Air Pollution," *Journal of the American Medical Association*, 287: 1132-1141.

[28] Palmer et al., 2005, "Reducing Emissions from the Electricity Sector, The Costs and Benefits Nationwide and for the Empire State," Resources for the Future Discussion paper 05-23, June; Palmer et al., 2007, "The benefits and costs of reducing emissions from the electricity sector," *Journal of Environmental Management*, 83(2007): 115-130.

states, many of which have such plants, with West Virginia, Pennsylvania, Ohio, Kentucky, and Indiana experiencing the highest mortality rates.[29] In fact, three of these states – Ohio, Pennsylvania and Indiana – are among the top four SO_2-emitting states in the country. As shown in Figure 2, which illustrates estimated ambient air quality ($PM_{2.5}$) improvement, Transport Rule benefits would be spread over a similar geographic region as current health impacts from coal-fired generation.

Figure 2
Transport Rule Impacts on 24-hour Small Particulate (PM2.5) concentrations in 2014



Source: EPA, 2010.

In addition to reducing premature deaths, estimated by EPA to be as high as 36,000 annually, the Transport Rule will also reduce non-fatal illnesses, particularly respiratory and cardiovascular conditions. The EPA estimated that the Transport Rule would reduce over 10 million of these non-fatal illnesses annually. These conditions include chronic and acute bronchitis, non-fatal heart attacks, asthma exacerbations, and other upper and lower respiratory symptoms.[30] While the sheer number of avoided respiratory and cardiovascular conditions would be far greater than the number of avoided premature deaths, the estimated benefit from avoiding

[29] These states have the highest mortality rates from coal-fired power generation as estimated by Abt Associates. Clean Air Task Force, "The Toll from Coal, An Updated Assessment of Death and Disease from America's Dirtiest Energy Source," September 2010.

[30] EPA, June 2010, pp. 4-5.

one of these incidents is dramatically smaller than the benefit of a single avoided death. As a result, estimated morbidity benefits account for 4 to 20 percent of total benefits, as opposed to 80 to 96 percent for reduced mortality.[31] However, as illustrated in Table 3, estimates of the morbidity benefits will tend to understate the economic benefit of reducing illnesses to the extent they rely upon "cost-of-illness" methods that only capture reductions in health care expenditures and/or improvements in worker productivity, but do not capture improvements in well-being, such as the value of avoiding pain, discomfort, and other negative effects.

Table 3
Categories of Benefits Estimated in EPA's Transport Rule RIA

Benefit Category		Examples of Health and Other Effects Estimated Use Alternative Approaches	Well-Being	Resource Saving (e.g., reduced health care costs, improved worker productivity)
Health	Mortality	Premature	Willingness-to-Pay	
	Morbidity	Chronic Bronchitis	Willingness-to-Pay	
	Morbidity	Non-fatal Heart Attacks Hospital Admissions (respiratory, cardiovascular) Acute Bronchitis		Cost of Illness
Amenity	Visibility	National Parks & Monuments	Willingness-to-Pay	

Health improvements not only enhance people's quality of life, but also lead to resource cost savings, through reductions in health care expenditures and greater work-productivity. As shown in Table 3, one estimate of the magnitude of these benefits is the sum of the individual benefits estimated using a cost-of-illness approach – \$3.7 billion annually, based on EPA analysis.[32] Similarly, Cicchetti estimates that the Transport Rule would provide benefits (avoided lost income) of \$5.92 billion annually due to reductions in lost workdays and health insurance costs.[33] However, as shown in Table 3, these estimates do not include resource savings from the reductions in mortality, chronic bronchitis and other conditions evaluated through willingness-to-pay methods, since these methods do not allow any resources savings to be distinguished from improvements in well-being. As such, any estimate of resource savings that excludes these values would tend to understate the true magnitude of these savings.

B. Costs

Achieving improvements in air quality under the Transport Rule requires directing resources – capital, labor, and materials – to actions that lower electric sector emissions, while still ensuring the continued reliability of electricity supply – that is, ensuring that there are sufficient generation and demand-response resources to meet customer's loads at all times.

[31] EPA estimates that the Transport Rule would create annual benefits of about \$8.3 billion from reduced morbidity, while Palmer *et al.* estimate that CAIR, which the Transport Rule will replace, has morbidity benefits of \$3.4 billion in 2010 and \$4.9 billion in 2020. These estimates reflect benefits associated with ozone and $PM_{2.5}$, but do not include benefits from reductions in coal-related mercury emissions that likely arise as an ancillary benefit of reduced SO_2 emissions.

[33] Cicchetti, Charles, "Expensive Neighbors: The Hidden Cost of Harmful Pollution to Downwind Employers and Business," 2010, p. 37.

1. Reducing Emissions from Energy Production

The electric sector can achieve NO_X and SO_2 emissions reductions through a variety of approaches, including expanded utilization of existing pollution-control equipment,[34] switching to coal with lower sulfur content,[35] installation of new pollution-control equipment, and switching to more efficient and/or lower-emitting generation sources. Most analyses find that each of these approaches would contribute in varying degrees to reducing SO_2 and NO_X emissions under the Transport Rule. However, estimates of the extent to which each of these alternatives would be used, and the associated costs of compliance, depend upon many assumptions, such as the cost of pollution control retrofit, the opportunity for higher utilization of existing pollution controls, constraints on further switching to lower sulfur coals, and the relative cost of alternatives to coal-fired power.

Many different post-combustion technologies are available to reduce emissions, with costs and effectiveness varying across these alternatives. Selective Catalytic Reduction (SCR) and Selective Non-Catalytic Reduction, which target NO_X emissions, and wet and dry Flue Gas Desulfurization (FGD), which target SO_2 emissions, are the most likely compliance alternatives to be deployed under the Transport Rule. Alternative technology options for smaller coal plants, however, such as dry sorbent injection (DSI) for SO_2 control, offer lower capital costs, and shorter construction times, and show promise for Transport Rule compliance.[36] Notably as well, to comply with other CAA regulations or state requirements, many coal-fired power generation facilities are already equipped with pollution-control equipment. Table 4 below shows that 70 percent of coal-fired generation capacity has NO_X or SO_2 controls.

Table 4
Existing National Generation Infrastructure

Existing Power Generation Capacity	Capacity (GW)	Percent of Total Installed Capacity	Percent of Coal-fired Capacity
Total Installed Capacity	1,122		
Total Coal-fired Generation Capacity	341	30%	
No control	103	9%	30%
FGD only	65	6%	19%
SCR only	58	5%	17%
FGD & SCR	115	10%	34%

Note: Coal-fired capacity figures reflect both existing and planned pollution controls.
Source: Credit Suisse, "Growth from Subtraction," September 23, 2010; Energy Information Administration, *Electric Power Annual, January 4, 2011.*

Installing new pollution controls involves a variety of costs, including labor to install and operate equipment, material inputs to equipment operation and construction, and capital to finance

[34] For example, EPA finds that, under the Transport Rule, an additional 40 GW of coal-fired facilities will choose to operate their FGD scrubbers year-round rather than for only a portion of the year, while year-round operation of SCR for NO_X control will rise by 51 GW. U.S. EPA, Regulatory Impact Analysis for the Proposed Federal Transport Rule, Docket ID No. EPA-HQ-OAR-2009-0491, June 2010, p. 258-259.

[35] The scope for further emission reductions from fuel switching is uncertain. Opportunities for significant cost savings may have largely been exhausted in complying with prior regulations.

[36] These also include low NO_X burners for NO_X control.

investments. In addition, operating pollution control raises the cost of producing power.[37] EPA estimates that 32.8 GW of FGD and 2.4 GW of SCR would be installed by 2020 to meet Transport Rule requirements (relative to a baseline without CAIR).[38]

Although potentially large in absolute terms, pollution control capital expenditures to comply with the Transport Rule would comprise a relatively small fraction of the aggregate capital expenditures anticipated in the coming decades as the industry enters into a new "investment cycle" to modernize grid infrastructure, address declining reserve margins, and adapt to enhanced environmental objectives. Awareness of the growing need for substantial capital investment is not new. For example, a 2008 study found that the electricity industry needs $1.5 trillion in new investment over the next two decades to replace and modernize aging infrastructure and meet growing demand.[39] By contrast, based on EPA estimates, capital expenditures needed to comply with the Transport Rule could range from $10 to $30 billion.[40]

In fact, pollution control investment made to comply with CAIR and pre-existing regulatory requirements, such as New Source Review settlements and state environmental policies, help reduce new investment needed to comply with the Transport Rule in coming years. For example, to comply with Phase I of CAIR and other requirements, between 2007 and 2009, plant owners installed FGDs on 57 GW of coal-fired generation and SCRs on 31 GW.[41] Regulatory requirements are also driving planned retrofits in future years. For example, one study reports that planned installations of 20 GW of FGD and 10 GW of SCR and SNCR between 2012 and 2015.[42]

Switching from coal to lower emitting fuels and alternative technologies can also reduce SO_2 and NO_x emissions.[43] If the relative cost of these lower emission alternative sources of

[37] For example, operating costs can increase due to labor requirements for pollution control equipment, materials costs (for example, sorbents injected into plant exhaust), environmental management costs (for example, waste disposal), and "parasitic" load that reduces a plant's effective output.

[38] EPA, IPM v.4.10 Model Runs, September 1, 2010, "TR Base Case v.4.10", and "TR SB Limited Trading v.4.10). Other studies, such as Credit Suisse (2010) and CRA (2010), have analyzed the effect of combinations of different EPA rules, but do not analyze the Transport Rule in isolation. NERC does not report estimated retrofits. North American Electric Reliability Corporation (NERC), "2010 Special Reliability Scenario Assessment: Resource Adequacy Impacts of Potential U.S. Environmental Regulations," Princeton, N.J., October 2010; Credit Suisse, "Growth from Subtraction," Equity Research, September 23, 2010; Charles River Associates (CRA), "The Reliability Implications of EPA's Proposed Transport Rule and Forthcoming Utility MACT," December 16, 2010;

[39] Chupka, Mark, et al., "Transforming America's Power Industry: The Investment Challenge 2010-2030," prepared for the Edison Foundation, November 2008.

[40] This estimate reflects EPA estimates of retrofit quantities and costs. EPA estimates of retrofits needed to comply with the Transport Rule (relative to a pre-CAIR baseline) at 32.8 GW of FGD and 2.4 GW of SCR retrofits. Capital cost estimates for pollution control vary depending upon many factors in EPA's IPM model. Our estimate assumes a range of unit costs: $385 to $817 per kW for FGD, and $147 to $258 per kW for SCR as reported in "illustrative" examples provided in IPM documentation. EPA, "Documentation for EPA Base Case v.4.10, Using the Integrated Planning Model," Office of Air and Radiation, August 2010, Tables 5-4 and 5-8.

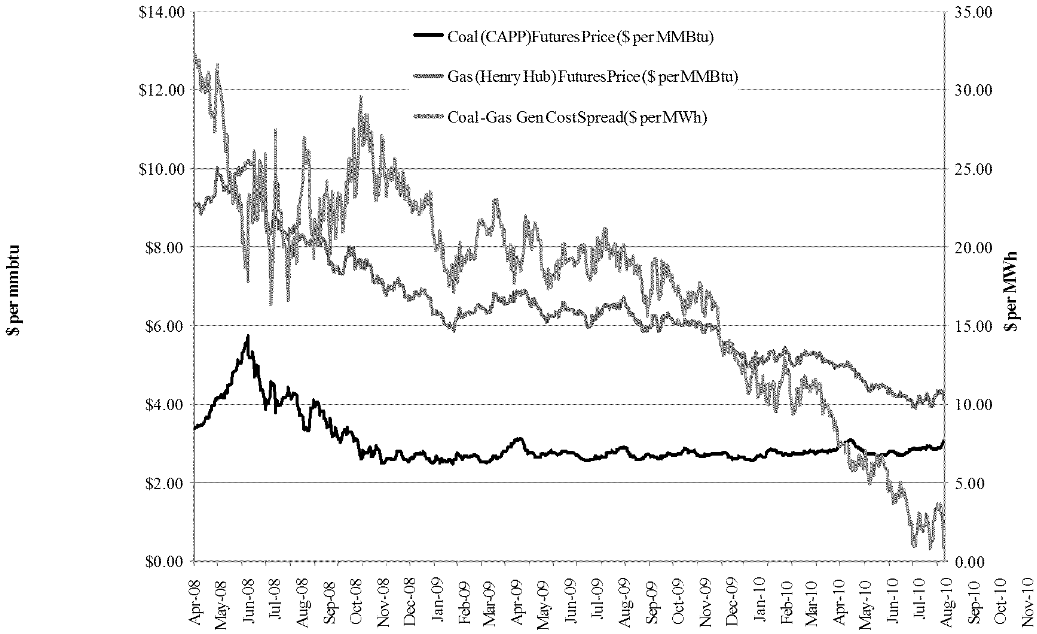
[41] EPA, NEEDS v.4.10 Database.

[42] Credit Suisse, 2010, Exhibit 46.

[43] If Transport Rule investments and responses increase electricity prices, consumers may reduce their energy use. Like substitutions in the electricity supply, these adjustments potentially lower the social cost of complying with new requirements.

electricity is favorable, substituting them for coal-fired generation can cost-effectively reduce emissions.[44]

Figure 3
Coal and Natural Gas 2011 Futures Prices and Coal-Gas Generation Cost Spread



Notes: Prices are for April 2011 delivery. The Coal-Gas Generation Cost Spread is the difference between the fuel costs needed to generate 1 MWh of power based on EIA heat rates (for new capacity) of 9,200 Btu/kWh for coal and 6,974 Btu/kWh for combined cycle gas.
Sources: SNL Financial - NYMEX Coal Futures, and NYMEX Henry Hub Futures, December 1, 2010.

Recent market trends and technology developments have substantially lowered the cost of transitioning from coal-fired generation to alternative power sources. In particular, technological advances in natural gas extraction have greatly expanded economically viable supplies from unconventional sources, including shale deposits, tight sands, and coal-bed methane.[45] Meanwhile, coal prices have gradually risen over the past decade.[46] As a consequence of these fuel price trends, and as shown in Figure 3, the gap between coal and gas prices has shrunk in recent years, which has made natural gas facilities increasingly competitive with coal-fired facilities.[47] Given these changing fuel price economics and evolving EPA regulations, some less efficient coal-fired

[44] For example, if a coal-fired facility generates power at $35 per MWH before installing and operating pollution-control equipment, and at $40 per MWh with pollution control, the compliance cost would be $5 per MWh. However, if the cost of a combined cycle natural gas facility is $37 per MWh, then increasing output from this plant and decreasing output from the coal plant can save $3 per MWh.

[45] Actual supplies will depend upon many factors, including EPA regulations of natural gas extraction. MIT Energy Initiative, *The Future of Natural Gas, An Interdisciplinary MIT Study*, Interim Report, 2010.

[46] For example, average coal prices rose 86 percent from 2000 to 2008. EIA, Annual Coal Reports.

[47] The competitiveness of particular plants will depend on many specifics, including fuel delivery costs (which are not reflected in Figure 1), plant-specific heat rates, and other operations costs, including pollution control equipment operation.

facilities have already chosen to retire even before full implementation of the Transport Rule.[48] Thus, increasing availability of low-cost natural gas can not only help lower the Transport Rule's compliance costs, but can also directly influence coal plant retirement decisions.

2. Maintaining Reliability

The electric industry has a responsibility to provide customers with reliable electric service at all times. To ensure that customers' loads can be met at all hours of the day, utilities must maintain sufficient resources (with appropriate operating characteristics) to meet anticipated peak electricity demands. These resources can include both physical generation capacity and demand response resources.

To account for load growth and retirement of older facilities, in most regions, new generation capacity must be added over time. If new regulatory requirements reduce available installed generation capacity, new resources will have to be added sooner, thereby increasing the discounted cost of maintaining sufficient capacity. Replacing lost resources traditionally required construction of new generation facilities. However, alternative options, particularly demand response, are now available and often cost-effective, and have been widely deployed to help grid operators ensure customer's loads are met without interruption.[49]

Given the combined effect of expanded supplies of low priced natural gas, new air regulations and other factors (e.g., aging facilities), some facilities may retire instead of installing and operating pollution-control equipment, thus reducing available generation capacity. Such a retirement decision may be economically rational if the likely future revenues in the electricity market would provide insufficient return on capital investments in the new equipment. New regulatory requirements could also reduce generation capacity if installing new pollution control equipment reduces a facility's net output (a "derating").[50]

These retirements and deratings do not themselves impose an economic cost; an economic cost is incurred when the lost capacity needs to be replaced earlier than would otherwise be necessary. Consequently, for regions with more resources than are needed to maintain reliability, the cost associated with retired capacity could be deferred for many years into the future. NERC estimates of generation capacity reductions under the Transport Rule, reflecting both potential facility retirement and de-ratings due to pollution-control equipment, suggest that EPA's preferred regulatory approach would lead to less than a three percent reduction in the nation's 341 GW of coal-fired capacity.[51] Furthermore, in many regions with excess capacity (i.e., more resources than needed to maintain reliability), the economic cost of replacing this lost capacity may be deferred for many years. For example, NERC finds that less than 10 percent of total projected reduced

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[48] Tierney *et al.* report that various utilities have recently announced the retirement of 4.9 GW of coal-fired power generation. Tierney, Susan, Michael J. Bradley, et al., "Ensuring a Clean, Modern Electric Generating Fleet while Maintaining Electric System Reliability," August 2010. Similarly, the most recent *State of the Market Report* from PJM's Independent Market Monitor identified over 11 GW of coal-fired power units at risk for retirements because they "did not recover avoidable costs even with capacity revenues." PJM, *State of the Market Report*, Vol. 1, March 11, 2010.

[49] Demand response includes many mechanisms by which customers decrease their electricity use in response to price or other signals. In recent years, demand response has grown in nearly all regions as system operators have targeted this resource through new programs and markets. ISO/RTO Council, "2009 State of the Markets Report," 2009.

[50] For example, the power demands of pollution control equipment can reduce a facility's effective capacity.

[51] NERC, 2010.

capacity (0.25 GW of 2.9 GW) would occur in regions that will be below their reserve margins in 2015.[52] This would mean that only 250 MW of new capacity would need to be installed between now and 2015 to maintain reliability as a consequence of the Transport Rule.

Moreover, the quantities of reduced capacity contemplated in these analyses are small compared to capacity expansions achieved in prior periods. For example, over the five-year period between 1999 and 2004, 177 GW of new capacity was installed in the U.S., more than 60 times NERC's Transport Rule retirement forecast.[53]

To ensure that reliability can be maintained as regions meet Transport Rule emission targets, planning and market mechanisms exist to develop sufficient resources in a timely and efficient manner to meet customer loads. For example, in many restructured regional electricity markets, utilities are required to obtain sufficient long term capacity obligations to meet their customers' loads. Capacity markets, such as PJM's Reliability Pricing Model and ISO New England's Forward Capacity Market, provide a mechanism for utilities to procure commitments from existing and new resources to meet their customers' needs. Both PJM's and ISO New England's capacity markets create incentives for new entry up to three years in advance of actual need.

Within traditionally regulated markets, to fulfill their legal obligation to serve their customers reliably, vertically-integrated utilities undertake long-range resource planning. Their efforts to develop new generation and demand resources are complemented by grid operators' regional planning, which is designed to identify and undertake transmission investments that mitigate reliability concerns, including those that may arise due to generation retirements.

In addition to these long-term market and regulatory mechanisms, various backstop mechanisms exist to maintain reliability should local or regional reliability concerns arise. In particular, Federal agencies and grid operators can prevent particular generation facilities from retiring if their retirement would create reliability concerns, particularly in localized areas. [54]

C. Aggregation of Costs and Benefits and General Equilibrium Effects

Calculating the net benefits of the Transport Rule is, in principle, a straightforward exercise of comparing the estimated benefits with the estimated costs. As shown in Table 2, estimates of the annual cost of the Transport Rule or the CAIR range from \$2.4 to \$7.1 billion.[55] Other studies analyzing the electric industry impacts of the Transport Rule individually or as one of many regulations often do not develop estimates of a key issue before policymakers – the Transport Rule's social costs. To the extent that industry impacts from these studies differ from those estimated by EPA, social costs estimates may similarly differ.[56] By comparison, estimates of Transport Rule or CAIR benefits range from \$20 to \$309 billion annually (in \$2009) – a significant

[52] NERC, 2010.

[53] CRA, 2010; NERC, 2010.

[54] *See* Tierney et al., 2010, p. 22-23.

[55] Many factors contribute to differences in estimates across studies, including differences in Transport Rule and CAIR emission targets. Because cost estimates in Palmer et al. also reflect compliance with a national cap on mercury emissions, they likely reflect costs unrelated to CAIR compliance and thus would tend to over-state the cost of complying with CAIR alone.

[56] For example, *see* NERC, 2010.

multiple of the corresponding cost estimates.[57] Thus, studies to date have concluded that the Transport Rule's benefits far exceed its costs. In fact, estimated Transport Rule costs are lower than estimated benefits even under more conservative assumptions about mortality impacts, as shown in Table 2, column [i].

Estimates of the national benefits and costs of any regulation, as shown in Table 2 for the Transport Rule, may not fully reflect regional differences in a policy's net benefits. Although the benefits of the Transport Rule will be spread across all eastern states (as shown in Figures 1 and 2), the costs are likely to be borne disproportionately in states relying heavily upon coal-fired power generation. Because of the potential for such regional differences, it is important to consider a policy's distributional consequences, as well as the aggregate benefits and costs it creates.

D. Timing of Regulatory Requirements

Determining the appropriate timing of new regulatory requirements requires an assessment of the economic tradeoffs among alternative compliance dates. Delaying the implementation of new regulatory requirements defers both the benefits created and the costs imposed. If compliance costs were independent of the timing of regulatory requirements (and if aggregate benefits exceed aggregate costs), then delaying the regulation only delays the society's enjoyment of the regulations' net benefits. By contrast, regulations implemented too quickly can raise the industry's transition costs by, for example, elevating equipment prices, creating labor shortages, requiring more costly, less efficient resources to meet near-term requirements, and temporarily reducing reliability.

However, appreciable transition costs from the Transport Rule appear easily avoidable given the anticipated quantity of pollution control retrofits estimated by EPA to comply with the Transport Rule's requirements, the limited quantity of coal-fired capacity expected to retire as a consequence of the Transport Rule, and excess capacity in many regions.[58] EPA's assessment indicates that compliance with the Transport Rule's Phase 1 2012 requirements would require limited, if any, incremental investment in pollution control.[59] While compliance with the Transport Rule's Phase 2 2014 requirements will necessitate installing some incremental pollution controls, the total quantity of retrofits anticipated through 2014, after reflecting already announced retrofits and retrofits needed to comply with the Transport Rule, appears to be no greater than the amounts

[57] All studies evaluated consider the Transport Rule's direct economic impacts. However, the Transport Rule could have broader economic impacts as the changes in prices arising from the compliance costs in the electricity sector, reduced costs in the health care sector, and other effects ripple throughout the economy. Given these potential effects, EPA's Transport Rule analysis also considers social costs within a general equilibrium framework, although these estimates only reflect changes in energy prices and not other price changes (e.g., health care).

[58] For example, Tierney et al. report that the national average utilization of natural gas combined-cycle capacity units was 33 percent in 2008, compared to 56 percent for coal-fired units, with a maximum regional utilization of 42 percent among Transport Rule regions. Tierney et al,, 2010, Table 4.

[59] EPA says that 2012 requirements are set to allow compliance through operation of existing scrubbers at full efficiency and through use of lower sulfur coal. FR Vol. 75, No. 147, August 2, 2010, p. 45281.

of equipment installed in recent years.[60] Moreover, as noted earlier, the industry has already undertaken retrofits to comply with CAIR and other regulatory requirements even as EPA developed the Transport Rule. Thus, it appears unlikely the 2014 requirements would lead to appreciable transition costs driven by the need either to install new pollution control equipment or to replace retired generation capacity.

E. Accuracy of Estimates of Benefits and Costs

Economic analysis of any major proposed regulation faces the tremendous challenges of forecasting the responses of industry and consumers to new regulatory requirements, the prices of basic resources (fuel, labor, and capital) in future time periods, and the value of future resource savings in, for example, health care.[61] Given these uncertainties, it is not surprising that *ex ante* estimates benefits and costs frequently differ from the actual benefits and costs arising during implementation. A systematic study of 25 environmental regulations in the US and abroad found that estimates of the costs of new regulations developed in *ex ante* economic analyses of proposed regulations have tended to overstate costs.[62]

Any tendency to overstate costs has been driven, in large part, by the emergence of new, unanticipated technologies that lowered compliance costs, particularly for regulations that provide compliance flexibility, including the use of economic incentives or market-based mechanisms. By providing compliance flexibility, regulation can create incentives to develop less costly compliance solutions, since regulated entities can capture the savings from using these more cost-effective technologies. For example, allowing coal-fired generation facilities flexibility in achieving SO_2 reductions under the Title IV SO_2 cap-and-trade system prompted scrubber and fuel switching innovations, resulting in SO_2 compliance costs significantly below original estimates.[63] The potential for DSI to contribute to Transport Rule compliance illustrates how technology choices for SO_2 reduction continue to evolve even today.

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[60] As noted earlier, from 2007 to 2009, 57 GW of FGD and 31 GW of SCR were installed. EPA, NEEDS v.4.10 database. The pollution control industry appears able to expand its capabilities and labor supply to some degree when there is sufficient demand. For example, Staudt finds that actual installation of pollution control exceeded EPA's assessment of industry capability performed during the CAIR rule-making. Staudt, James. E., "Availability of Resources for Clean Air Projects," Andover Technology Partners, October 1, 2010.

[61] Accounting for these uncertainties is an important part of a well-developed benefit-cost analysis. OMB guidelines during the George W. Bush administration called for explicit analysis of uncertainty (e.g., Monte Carlo analysis) for important regulations. *See*, Jaffe, Judson and Robert Stavins, "On the Value of Formal Assessment of Uncertainty in Regulatory Analysis," *Regulation and Governance* 1(2007): 154-171.

[62] Harrington, Winston, Richard D. Morgenstern, and Peter Nelson, "On the Accuracy of Regulatory Cost Estimates," Resources for the Future Discussion Paper 99-18, January 1999. OMB performed a similar analysis, finding that "… U.S. Federal agencies tend to overestimate both benefits and costs, but they have a significantly greater tendency to overestimate benefits than costs." Office of Information and Regulatory Affairs, "Validating Regulatory Analysis: 2005 Report to Congress on the Costs and Benefits of Federal Regulations and Unfunded Mandates on State, Local, and Tribal Entities", OMB, 2005.

[63] For example, *see*, Ellerman, A. Denny, Paul L. Joskow, Richard Schmalensee, Juan-Pablo Montero, and Elizabeth M. Bailey, *Markets for Clean Air: The U.S. Acid Rain Program*, New York: Cambridge University Press, 2000; Carlson, Curtis, et al., "Sulfur Dioxide Control by Electric Utilities: What Are the Gains from Trade?," *Journal of Political Economy* 108(6): 1292-1326.

Although the Transport Rule limits interstate emission trading, it nonetheless continues to provide electricity generators with significant flexibility in SO_2 and NO_X compliance. Thus, by its design, the Transport Rule provides incentives for technological innovation that potentially reduce costs below initial estimates.

IV. Distributional Economic Impacts

Along with the aggregate benefits and costs of a proposed regulation, the regulation's likely economic impact on various groups and locations is an important concern for policy makers, as well as for the affected stakeholders. For the Transport Rule, key impacts include local and regional changes in electricity rates and employment – and in economic growth more generally. Given the great interest in reducing unemployment in the current recession, employment and economic growth are critical to policy discussions.

Understanding the implications of changes in electricity rates, jobs, or other economic factors requires recognition that these changes have different consequences for different participants in the economy. Electric rate increases are generally negative for energy consumers, whether households or businesses, but may be essential for energy companies to recover some of the costs of new regulations. Likewise, increased job opportunities are good for workers, particularly in times when unemployment is high and wages are stagnant.

A. Impacts on Electricity Rates

By spurring new investment and raising the costs of producing electricity, new environmental regulations can increase retail electricity rates. However, the size of any rate increases will depend upon many factors, including the stringency of new requirements, the costs of available alternative compliance approaches, and the fuel mix in and structure of the markets. Because of the complexity of these interrelated factors, and the importance of details specific to individual regulations and regions, it is very difficult to generalize about how new environmental regulations will affect electricity rates.

While many of the Transport Rule's emission reduction costs will be passed through to customers in the form of higher electricity rates, the actual changes in rates will depend upon two key factors. First, the impacts will depend upon the market and industry structure that serves each customer. Industry restructuring over the past decade has resulted in a patchwork of market and regulatory structures that will produce different outcomes for different consumers. At one extreme, vertically integrated utilities regulated under the traditional cost-of-service regime, which own about three-quarters of coal-fired capacity, will typically be able to pass through all prudently incurred investments and operating costs into their retail rates over an extended period of time.[64] However, the need for such new investment will depend on many factors, including alternatives to retrofitting existing generation or building new generation, such as purchasing replacement power on wholesale markets. Thus, customers served by these utilities are likely to bear the full cost of compliance through rate increases. By contrast, owners of merchant power facilities, whose prices

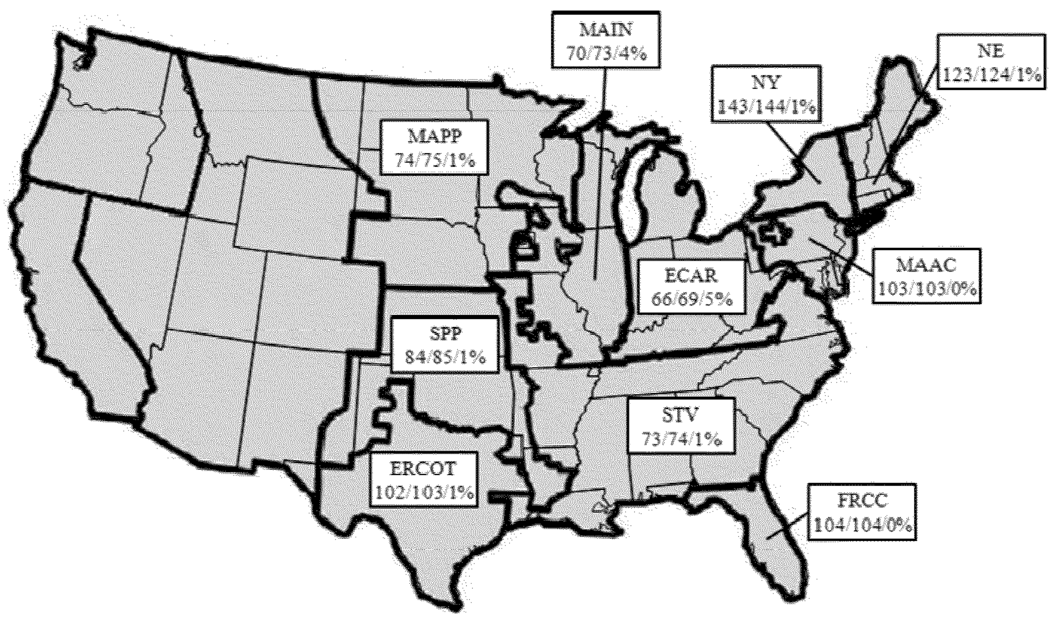
[64] Credit Suisse, p. 25.

are determined in competitive wholesale markets rather than regulated on a cost-of-service basis, will find it more difficult to pass through the costs of regulatory compliance.[65]

The second key factor affecting rate impacts is geography. Because electricity is not a commodity that is easily stored or transported long distances, the costs and resulting rates to customers also depend greatly on the character of the regional system used to serve each customer. Under the Transport Rule, regional variation in rate impacts may arise because of differences in reliance on coal-fired power generation, the extent to which existing facilities have already invested in pollution-control equipment, and the stringency of state emission budgets.

While rate increases are likely to be greatest in the states most reliant upon coal-fired generation, these states now typically enjoy among the lowest electricity prices in the country. As shown in Figure 4, EPA estimates of rate impacts from the Transport Rule, which vary from 0% to 5% of existing rates across the 10 regions analyzed by EPA, tend to be greatest in the regions that currently enjoy the lowest electricity rates.

Figure 4
EPA Estimates of Regional Electricity Rates With and Without the Transport Rule (TR)
(Average Rate without TR / Average Rate with TR / Percent Change) (Rates in $ per MWh)



Source: EPA, 2010.

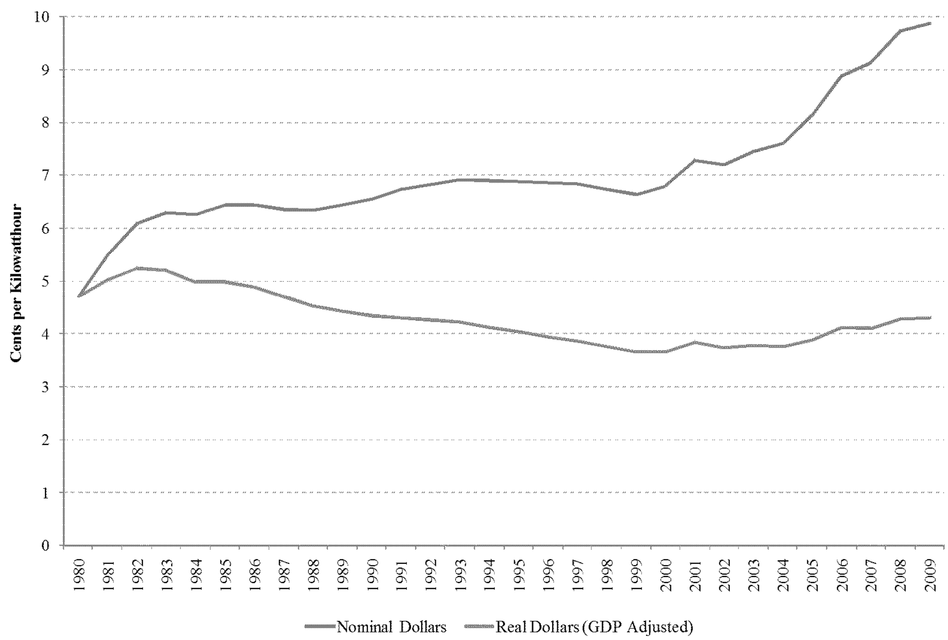
Recent downward trends in wholesale market prices may also help mitigate any Transport Rule rate impacts. Driven in large part by lower natural gas prices and lower demand, wholesale energy prices have fallen recently. For example, load-weighted energy prices in PJM were 45 percent lower in 2009 than in 2008, and remained 31 percent below 2008 averages through the first

[65] The wholesale markets into which these facilities sell their power have no direct mechanism to allow recovery of investments in pollution-control equipment. Further, the cost of operating equipment can only be recovered to the extent that wholesale prices rise to cover new variable costs. However, such price increases will only occur if the marginal price-setting units in wholesale markets are affected by new regulations.

three quarters of 2010.[66] While economic recovery will put upward pressure on wholesale prices, most analysts agree that sustained low natural gas prices are likely to continue to place downward pressure on utility rates for many years. In addition, in regions where coal-fired generation does not set the market price, these declining prices serve as another indicator of the opportunity offered by low-cost natural gas supplies and other power sources to lower the cost of achieving emission reductions.

Changes in energy prices can have adverse consequences for households by increasing the share of household budgets that must be devoted to electric utility bills. These changes in rates can be potentially regressive since low-income households typically spend a larger share of their incomes on electricity.[67] However, even for the lowest income households, electricity bills represent on average less than 5 percent of all household expenditures. In addition, many utilities provide programs that subsidize the electricity rates offered to those in the lowest income brackets and can thus shelter particularly vulnerable families from any rate impacts of new regulations. Moreover, as shown in Figure 5, while electricity prices have been rising in recent years, the real, inflation-adjusted cost of electricity is still lower than in the early 1980s.

Figure 5
Real and Nominal Electricity Prices, National Average



Note: Inflation-adjusted, real dollar estimates are adjusted based upon the GDP price deflator.
Source: Bureau of Economic Analysis.

Increases in electricity rates also potentially have an adverse effect on businesses by raising their costs of production. Because the Transport Rule affects only electric generating units, any

[66] Monitoring Analytics, *2009 State of the Market Report for PJM*, Volume 1, March 11, 2010; Monitoring Analytics, *Q3 2010 State of the Market Report for PJM*, November 15, 2010.

[67] In 2009, expenditures on electricity were on average 2.8 percent of total household expenditures and 4.2 percent of total expenditures for households with after-tax incomes less than $15,000. Consumer Expenditure Survey, U.S. Bureau of Labor Statistics, October, 2010.

new costs would arise only for businesses that rely heavily on electricity. Moreover, as we discuss below, these rate impacts are only one of many potential ways that new regulations may affect a region's level of economic activity (and jobs). Finally, only small rate impacts are anticipated from the Transport Rule, and these could be partially or more than fully offset by other drivers of regional economic change, including relaxation of regulatory requirements in many noncompliance regions.

B. Economic Growth and Employment

With today's high unemployment rates and sluggish economic recovery, policymakers and the public are particularly interested in the job effects of new environmental regulations. Will new regulations create or destroy jobs? Where and in what sectors?

In good economic times, when the workforce is fully or almost fully employed, using labor to meet new regulatory requirements both raises the costs of regulated goods and means that fewer workers are available to do other productive things in the economy. By diverting scarce labor resources away from other activities, the use of labor thus imposes an opportunity cost on society, which should be considered alongside the capital costs of pollution reduction.

However, in difficult economic times, such as today's, when unemployment is high, some workers used to meet new regulatory requirements may have otherwise been unemployed or underemployed. Thus, using their labor to implement the regulation imposes lower costs on society. Moreover, through indirect effects, environmental regulation may spur economic activity and job growth in sectors not directly affected by the regulation, but which provide goods and services for those sectors.

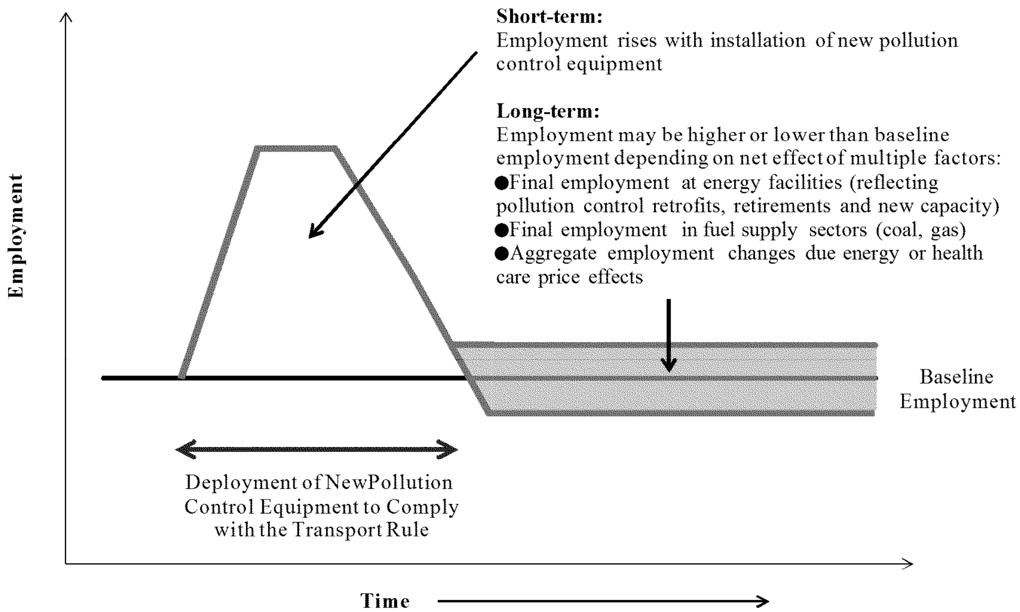
The mechanisms that drive job impacts reflect the various economic adjustments made in response to the new regulations. Direct responses to regulation will lead to short-term job gains from the manufacture and installation of new pollution control equipment to comply with the regulation. In the long run, adjustments in employment will depend upon how the power sector industry adjusts to the new regulatory requirements, as well as the indirect upstream and downstream effects of those adjustments on the rest of the economy. These direct and indirect impacts can vary in their magnitude over time, and across regions and sectors.

The particular nature of the regulation can also affect employment impacts. Since environmental improvements are often achieved through regulations on multiple entities in multiple locations, more stringent regulations in one location potentially may relax regulatory requirements on other entities in other locations. For example, by reducing emissions from upwind sources, and helping downwind regions attain NAAQS compliance, the Transport Rule may relax regulatory requirements on sources in those downwind regions.

Moreover, because these various adjustments can lead to many *offsetting* direct and indirect effects, which can vary across regions and sectors, determining the net employment effect is challenging. Consequently, estimates of partial or localized employment effects can paint an inaccurate picture of net employment impacts if not properly placed in a broader economic context.

Employment impacts from the Transport Rule are also likely to vary significantly over time. In the short run, compliance with the Transport Rule will likely lead to short-term job gains arising from the design, manufacture and installation of pollution controls.[68] Various estimates of the employment impacts associated with infrastructure installation suggest that these impacts could be significant with a large share of these immediate job gains occurring in regions where new equipment is installed. Moreover, while these job impacts would be temporary, they could also stimulate the broader economy and employment.

Figure 6
Illustrative Employment Impacts of the Transport Rule



Note: The figure provides a stylized depiction of Transport Rule employment impacts and does not reflect a quantitative assessment, such that the relative magnitude of depicted impacts reflects likely impacts.

While employment is likely to rise in the short run, in the long run, employment could either increase or decrease depending on direct changes in electricity generation, indirect effects as these changes ripple through the economy, and the relaxation of regulatory requirements as downwind regions come into NAAQS compliance. These impacts would also vary significantly across regions. In upwind regions subject to the Transport Rule, while some employment may be lost as a consequence of coal-fired generation retirements, these losses will be offset – at least partially and potentially more than fully – by employment gains from operating pollution control equipment and staffing the new generation facilities needed to replace any retired capacity.

[68] The installation of pollution-control technology may require a substantial amount of labor relative to the number of employees otherwise working at a power plant. For example, one study estimates that the manufacture and installation of FGD creates employment of 848-1,001 annual full-time equivalents (Industrial Economics, 2010). Assuming two years to install the unit, this means about 400 to 500 jobs. This same study estimates that 103 permanent workers are needed to operate and maintain this equipment. By contrast, the National Commission on Energy Policy found that 1 GW of coal-fired capacity requires 100 to 300 employees. See Price, Jason *et al.*, "Employment Impacts Associated with the Manufacture, Installation, and Operation of Scrubbers," Industrial Economics Memorandum, January 15, 2010; National Commission on Energy Policy's Task Force on America's Future Energy Jobs, Final Report.

In "downwind" regions, employment may rise as the Transport Rule brings these regions into attainment with NAAQS, thus allowing them to relax the more stringent emission standards imposed on non-attainment regions.[69] For example, new stationary sources in noncompliance regions must meet standards based on the Lowest Achievable Emission Rate (LAER), which are more stringent than the alternative Best Available Control Technology (BACT) standards. In addition, new sources in nonattainment regions must offset all (or even more than all) emissions through the purchase of emission offsets. The aggregate and cumulative effect of these more stringent requirements can be significant.[70]

In addition to relaxing existing requirements in noncompliance regions, the Transport Rule can also avoid the need to impose further requirements in these regions to help bring them into compliance. Moreover, the costs of achieving emission reductions through the Transport Rule are generally less costly than alternatives measures targeting non-electricity in-state sources. For example, EPA notes that the cost of SO_2 reductions by non-electricity sources ranges from $2,270 to $16,000 per ton of SO_2, compared to a maximum of $2,000 per ton for upwind electricity sources.[71] These differences in the cost-effectiveness of alternative means of reducing emissions not only have distributional consequences across regions, but also have consequences for aggregate national costs of bringing all regions into compliance with air quality standards.[72]

In addition to these direct effects on upwind and downwind regions, the Transport Rule could lead to job impacts through the price effects identified in earlier sections. For example, the Transport Rule would likely raise prices for electricity (particularly in regions heavily reliant on coal), and lower prices for health insurance by varying degrees across eastern states. The net impact of these adjustments on any given state is unclear, may vary across industries depending on the intensity of their electricity use, but is likely to be limited given the small price changes anticipated as a consequence of the Transport Rule.

V. Conclusion

As EPA undertakes the series of rulemakings affecting the electric utility sector, we believe the public interest requires that the Agency carefully assess all of the regulations' economic impacts – both in aggregate and across sectors and regions.

This paper provides a guide to understanding the appropriate analytical framework for considering these impacts, and a lens to assess the economic consequences of the Transport Rule. Through our limited examination of studies assessing various anticipated effects of the Transport Rule, we highlight several important points:

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[69] *See,* Cicchetti, 2010, pp. 33-35.

[70] Greenstone estimates that counties out of attainment with the CAA lost approximately 590,000 jobs and $127 billion ($2009) in output over the first 15 years of implementation of the CAA (compared to counties in compliance with the CAA.) Greenstone, Michael, "The Impacts of Environmental Regulations on Industrial Activity: Evidence from the 1970 and 1977 Clean Air Act Amendments and the Census of Manufacturers," *Journal of Political Economy* 100(6). *See, also* Becker, Randy and Vernon Henderson, "Effects of Air Quality Regulations on Polluting Industries," *Journal of Political Economy* 108(2):379-421.

[71] F.R. Vol. 75, No. 147, p., 45281.

[72] Any conclusions about cost-effectiveness of alternative approaches to emission reductions must reflect differences in the benefits created by reducing emissions from alternative sources given each source's specific geographic location and the air transport of emissions to downwind populations.

1. Existing studies providing estimates of the Transport Rule's benefits and costs consistently find that benefits outweigh costs, on a national basis, often by a wide margin.
2. Existing studies' estimates of the Transport Rule's health benefits and their conclusions that the Transport Rule would likely produce positive net benefits appear robust to changes in several key modeling assumptions.
3. Given electric infrastructure changes forecast by several studies, the proposed timing and requirements in the Transport Rule appear unlikely to raise the national costs of implementation significantly.
4. Expanded supplies of low-cost natural gas and currently underutilized labor supply to help install pollution control equipment may well lower the social cost of the Transport Rule and mitigate the impact on electric rates.
5. Although designed to address "upwind" states' power plant pollution impacts on "downwind" states, this characterization may misrepresent the geographic distribution of the Transport Rule's benefits and costs. While the Rule's economic costs are most likely to be borne in upwind states relying heavily on coal-fired power, because of reduced emissions, these states also would likely receive substantial benefits from the Rule, largely in the form of improved health outcomes.
6. Employment will likely rise in the short run as a consequence of the Transport Rule, due largely to investment in new pollution controls. In the long run, the net employment impacts could be either positive or negative, depending upon a number of economic factors, including potential increases in energy prices, potential declines in health insurance costs, and changes in labor requirements to operate the electric industry's infrastructure, as well as changes in the aggregate level of unemployment.

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Viewpoint

Firm performance and employment in the EU emissions trading scheme: An empirical assessment for Germany

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Abstract

This paper empirically investigates the role of the EU Emissions Trading Scheme (EU ETS) for firm performance and employment in Germany. We provide an overview of relative allowance allocation within the EU ETS as well as an econometric analysis for a large sample of German firms covered by the scheme in order to assess the impacts of EU emissions regulation on both firm revenues and employment. The dataset indicates that the EU ETS was in an overall long position in 2005, although allowance allocation was very heterogeneous across member states. Our econometric analysis suggests that, within the first phase of the EU ETS, relative allowance allocation did not have a significant impact on firm performance and employment of regulated German firms.

Keywords: EU ETS; Competitiveness; Employment

1. Introduction

In 2005, the European Union Greenhouse Gas Emissions Trading Scheme (EU ETS) was launched (European Union, 2003). The scheme represents a cornerstone of the efforts by EU member states to fulfil the emissions reduction targets under the Kyoto Protocol. This agreement requires European countries to reduce their greenhouse gas emissions on average by 8% until 2012 compared with 1990 emissions levels (UNFCCC, 1997). The EU ETS covers European producers in four sectors, namely energy (e.g. electric power, oil refinement), production and processing of ferrous metals, minerals (e.g. cement, glass), as well as pulp and paper. The ETS currently covers almost half (46%) of the total CO_2 emissions of EU countries. While in the scheme's first phase (2005–2007) almost all emission allowances are grandfathered by means of National Allocation Plans (NAPs) of each member state and only up to 5% may be auctioned, in the second phase (2008–2012) the auctioning limit rises to 10%. Furthermore, the amending directive linking the EU ETS with the Kyoto Protocol's project-based mechanisms enables EU companies to generate emissions reductions by means of the Clean Development Mechanism (CDM) or Joint Implementation (JI) (European Union, 2004).

Since its initiation, the EU ETS has been accompanied by discussions on potential losses in competitiveness in international markets of companies that are covered by the EU ETS legislation.[2] Against this background, this paper presents a first empirical assessment of the effects of the EU ETS on firm performance, i.e. competitiveness, and employment. Following Balassa (1962), we define competitiveness as a firm's ability "to sell on foreign and domestic markets" and approximate this ability by firms' market revenues. We rely both on real-world data on allocated allowances and verified emissions for the first trading period from the EU Community Independent Transaction Log (European Union, 2007) and on economic firm-level data from two comprehensive databases.

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[2]For a recent overview on model-based assessments of costs and competitiveness effects of the EU ETS, see Oberndorfer and Rennings (2007).

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Previous quantitative studies have assessed the efficiency aspects and competitiveness implications of the EU ETS predominantly in numerical modeling frameworks. Böhringer et al. (2005) show that the exclusive coverage of energy-intensive installations by EU ETS implies that—in the absence of the potential use of CDM and JI—the remaining industries have to be regulated by complementary abatement policies in order to meet the national Kyoto targets. Such a hybrid emission regulation can cause large inefficiencies within EU economies, but may also worsen the prospects of linking the EU ETS to emerging emissions trading schemes beyond Europe (see Anger, 2007). Unlike employment aspects of the EU ETS, competitiveness implications of the current European trading scheme have been analyzed in numerical model frameworks (Kemfert et al., 2005; Klepper and Peterson, 2004; Peterson, 2006). The sectoral competitiveness implications of allowance allocation under the EU ETS have been assessed both for the European electricity industry (Neuhoff et al., 2006) and for the cement sector (Demailly and Quirion, 2006). Enlarging the purely European perspective, Alexeeva-Talebi and Anger (2007) assess both the economy-wide and the sectoral competitiveness effects of linking the EU ETS internationally to emerging trading systems outside Europe (such as Japan, Canada or Australia) within an applied general equilibrium model framework.

The previous empirical literature on emissions regulation under the EU ETS is rather scant. Analyzing the verified emissions of the participating installations as well as the respective allowances allocated, Ellerman and Buchner (2006) conclude that ''over-allocation occurred and that its magnitude may have been as much as 100 million EU allowances''. Kettner et al. (2007) present similar findings, suggesting that in the first EU ETS trading year the scheme was in a long position regarding emissions allowances. Moreover, to date there is no empirical contribution available assessing the competitiveness or employment impacts of emissions allocation under the EU ETS. Our paper aims at starting to fill this gap. In this respect, the contribution of this analysis is twofold: Relying on installation-level allocation data from the EU Community Transaction Log in 2005, we (i) descriptively assess the relative allowance allocation under the EU ETS at the national level and (ii) econometrically test for competitiveness and employment impacts of the EU ETS for a large sample of German companies.

The present article is structured as follows. Section 2 summarizes the empirical literature. Section 3 discusses the relative allowance allocation in Europe as well as the data underlying the empirical analysis for Germany. Section 4 presents the econometric assessment and Section 5 concludes.

2. Literature review

The necessity of environmental regulation is mainly based on the reasoning that there are social costs of negative externalities such as pollution. However, strict environmental regulation is often accused of harming the competitiveness of the affected sector or firm. Such adverse economic effects (and especially effects on competitiveness) of environmental regulation are challenged by the so-called Porter hypothesis, suggesting that environmental regulation provides incentives for companies to innovate and that these innovations can stimulate economic growth and competitiveness of the regulated country (Porter and van der Linde, 1995).

In the context of competitiveness and employment, an important characteristic of emissions trading schemes is the choice of the underlying allocation method. There are several studies dealing with this issue: Demailly and Quirion (2007) quantify the impact of the EU ETS on production and profitability as two dimensions of competitiveness for the iron and steel industry. They find that competitiveness losses for this sector are small but are significantly determined by pass-through rates and the updating of allocation rules. While emissions-based updating should be avoided as it creates perverse investment incentives, output-based updating has ambiguous competitiveness effects—softening production losses, but reducing the likely gains in earnings before interests, taxes, debt and amortization. Böhringer and Lange (2005) investigate the trade-off between compensation and economic efficiency for output- and emissions-based allocation rules in an international emissions trading scheme. They find that the output-based rule not only induces substantially lower efficiency losses than the emissions-based rule, but also performs better in ameliorating adverse production and employment effects for energy-intensive industries. Fischer and Fox (2007) present a welfare analysis of alternative emissions allocation rules within a domestic US emissions trading scheme, focusing on sectoral and international leakage as generated by restricted sectoral coverage of domestic ETS and unilateral action. They find that, given domestic and international leakage, output-based allocation of emissions permits to the covered sectors is preferable to auctioned permits in welfare terms, even when allowing for pre-existing tax distortions. Moreover, grandfathered permits generate the highest welfare costs of emissions regulation.

Our empirical literature review focuses on competitiveness, as the empirical literature on employment effects of environmental regulation is rather scant. One exception is Golombek and Raknerud (1997), who empirically assess the employment effects of imposing environmental standards on polluting firms. Using Norwegian data they find that, for two out of three manufacturing sectors, firms under strict environmental regulations had a higher tendency to increase employment and a lower tendency to exit than firms under weak or no environmental regulation.

Empirical analysis of the effects of environmental regulation on competitiveness or, more general, economic performance of firms or sectors is rather rare, too, as truly exogenous measures are often barely accessible. Pickman (1998), Brunnermeier and Cohen (2003) as well as Jaffe and Palmer (1997) use US Pollution and Abatement Costs and Expenditures (PACE) as a proxy of the stringency of environmental regulation in order to test for the innovation effects of US industries. However, such costs may depend on other factors such as the response to regulation, as well as the right measurement and exact self-report of firms and industries. Therefore, it is unclear whether compliance costs under- or overstate true regulation costs (Brunnermeier and Cohen, 2003). Pickman (1998) as well as Brunnermeier and Cohen (2003) find evidence that those costs positively affect innovation, while the results of Jaffe and Palmer (1997) do not confirm such causal relationship. What is more, it is controversial if a positive effect of environmental regulation on innovation (or even environmental innovation) would imply a positive competitiveness record of environmental regulation, as e.g. opportunity costs (e.g. other investment or conventional innovations that have not been realized due to the burden of regulation costs) are neglected in such a setting.

Such problems do not arise in event studies on environmental regulation. Such studies measure the impact of environmental regulation on stock returns of firms (possibly) affected. They often only compute short-term financial market reactions, however. Furthermore, they hinge on the assumptions of efficient financial markets and of no anticipation of regulation by the market actors, which may often be very crucial for the interpretation of the results computed. Butler and McNertney (1991) consider the effect of elections, namely the 1982 state-wide gubernatorial elections in six US states. These states were identified as those where the election results were uncertain and expected to affect environmental regulation for energy utilities. The study shows that in those states in which the victory of a democratic governor was most unpredictable significantly negative cumulative abnormal returns arise. Blacconiere and Northcut (1997) consider the impact of the US Superfund Amendments and Reauthorization Act (SARA) of 1986 on stock returns for corporations from the chemical industry, finding significant negative cumulative abnormal returns only for 17 out of 26 SARA-related events analyzed. Two more recent studies consider the effect of the US Clean Air Act Amendments of 1990 on stock returns for energy utilities (Diltz, 2002; Kahn and Knittel, 2003). Both studies cannot show sharp financial market reactions. Oberndorfer and Ziegler (2006) find that the German phasing out of nuclear energy (similarly to Butler and McNertney (1991), measured by the victory of the acting government with participation of the Green party in the 2002 German Federal Elections) had at least no general negative short- and mid-horizon effect on the economic performance of energy corporations. As far as the EU ETS is concerned, there are not yet empirical contributions available that measure competitiveness impacts of the introduction of and allocation inside of the scheme.

All in all, most of the existing studies find only weak evidence of an effect of environmental regulation on firm performance. Furthermore, all groups of approaches tackling the question about performance—competitiveness—impacts of environmental regulation have their idiosyncratic flaws: While innovations do not represent an ideal competitiveness indicator and the use of compliance costs as a proxy for regulation is not uncontroversial, most event studies only focus on short-term financial market reactions given environmental regulation. For the EU ETS, no empirical contribution on competitiveness impacts is available yet. Preliminary and descriptive evidence, however, suggests that the scheme is characterized by a relatively generous emissions cap compared with verified emissions.

3. Data and variables

In this section, we present the data basis underlying the emissions allocation within the EU ETS. This is done by firstly giving an overview over (relative) allocation at the national level for all EU ETS countries. In a second step, we present the data basis used for our empirical policy assessment of employment and competitiveness effects associated with EU ETS relative allowance allocation in Germany.

3.1. EU ETS data

The 2005 allocation data were extracted from the EU Community Transaction Log (European Union, 2007). The allocation factor measures the allocation of EU emissions allowances relative to the actual emissions of the respective entity and is calculated as the quotient of allowances allocated to the verified emissions. The allocation factor thus shows the relationship between the amount of allocated allowances and actual emissions, i.e. an allocation factor larger than 1 suggests that an entity has received allowances that exceed its emissions, while an allocation factor smaller than 1 suggests that the respective entity either has to buy additional emissions allowances or abate some of its emissions in order to comply with EU ETS regulation. In this context, one problem may be that verified emissions do not stem from a pre-EU ETS period (this emissions data is actually not available) but from 2005, and are thus of ex-post nature. Therefore, relative allocation cannot be distinguished from actual early abatement in 2005 and also the allocation factor has an ex-post character.[3] First evidence, however, suggests that abatement in 2005 remained relatively low, so that the allocation factor should be at least a very good indicator for relative allocation (Ellerman and Buchner, 2006).

[3] Moreover, note that the allocation factor is dependent on factors such as stochastic variations in weather, production, energy prices, or other variables affecting emissions.

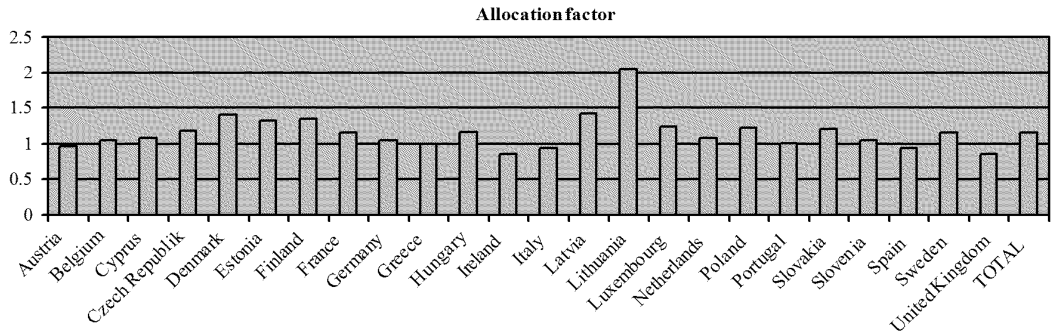

Fig. 1. Allocation factors at an aggregate national level for EU ETS countries (source: European Union, 2007; own calculations).

Fig. 1 shows the allocation factor, aggregated at the national level and based on disaggregated installation level data from the EU Community Transaction Log, for all EU ETS countries. Our aggregated data rely on 10,276 installations, covering the entire set of identifiable EU ETS installations. It indicates that companies in some countries—e.g. Ireland and the UK—have received fewer allowances than their respective emissions, while companies from other countries have received a large relative amount of emissions allowances given their actual emissions. Noteworthy in this respect is Lithuania, with its companies having received allowances for more than twice of their actual emissions. For the EU ETS level, the data extracted from the EU Community Transaction Log suggest that, in 2005, the scheme as a whole was in a long position. Furthermore, it already indicates that the relative allowance allocation enormously differs across single entities. This is in line with the findings of Kettner et al. (2007).

More specifically, Kettner et al. (2007), consistently with our calculations, identified Lithuania as the country exhibiting the biggest ''net long'' position and Ireland and the UK as countries exhibiting the biggest ''net short'' position within the scheme. Additionally to our calculations, however, they provide information on long and short positions at the sector level, which is not in the focus of our analysis.

3.2. German sample

In the framework of an empirical analysis for Germany, we want to assess the impact of relative allocation of EU emissions allowances on competitiveness and employment at the firm level. The econometric analysis can only be conducted within a case study for Germany, as economic variables that could indicate the development of competitiveness and employment at the firm level up to 2005 were not available to us for all EU countries. Still, the econometric analysis may offer important insights into the economic effects of the EU ETS in Europe as a whole, as Germany is the most important country within the EU ETS, its companies representing about 24% of all allowances allocated. To our knowledge, our approach represents the first ex-post analysis of the economic impacts of the EU ETS.

For the purpose of this empirical investigation, EU ETS allocation data stemming from the Community Transaction Log (European Union, 2007) (i.e. the allocation factor, see above) were aggregated at the firm level for Germany. The relative emissions allocation data were subsequently matched with economic data from the CREDITREFORM database. Sectoral (indicator) variables were generated according to the four-digit NACE industry codes that are contained in the AMADEUS database. Our sector classification includes the business, electricity, energy, mining, coke & petroleum, pulp & paper, and (other) manufacturing sectors. For more detailed information on the economic, sectoral and emissions data employed in this analysis, please refer to Appendix A.

All in all, given our economic data, 419 German firms covered by the EU ETS could be analyzed in the empirical framework. Table 1 gives first information on the 2005 data from the Community Transaction Log and the CREDITREFORM database. It shows that, on average, the companies included in the empirical analysis have been, in comparison with all German EU ETS participating firms, relatively highly allocated with EU emissions allowances. While for Germany as a whole the allocation factor is 1.04, for the sample analyzed it is 1.24. Furthermore, the economic data indicate that, in 2005, the decisive year of our analysis, the firms included have, on average, under circumstances of shrinking revenues, reduced their number of employees. More than a quarter of our sample firms stem from the manufacturing sector; mining and coke & petroleum firms, in contrast, are very infrequent here. In all, 153 firms could not be classified in our sectoral classification (for an overview over the sectoral distributions, see Table A1).

4. Econometric analysis for Germany

4.1. Estimation approach

An econometric analysis is the only means to empirically measure the impact of relative allocation of EU emissions allowances on competitiveness and employment. In the

Table 1
Descriptive statistics of German firm data

Variable	No. obs.	Mean	Std. dev.	Min	Max
Allocation factor	419	1.24	0.57	0.26	5.95
Allowances allocated	419	603,961.5	4,795,754	272	9.02e + 07
Verified emissions	419	579,399	4,862,883	50	9.12e + 07
Revenues 2005ffi2004	419	ffi139.43	3982.08	ffi79,678.02	8361.04
Revenues 2004ffi2003	419	77.01	507.40	ffi320.02	8539.71
Revenues 2003ffi2002	419	ffi18.92	1917.70	ffi21,191.99	18,451.61
Revenues 2005	419	972.32	4423.90	0.16	56,172.84
Revenues 2004	419	1111.75	7340.71	0.16	135,850.90
Revenues 2003	419	1034.74	7080.48	0.16	131,569.00
No. employees 2005ffi2004	419	ffi413.08	5367.39	ffi73,336	22,660
No. employees 2004ffi2003	419	424.90	5328.04	ffi3508	72,712
No. employees 2003ffi2002	419	ffi80.21	705.73	ffi6899	3850
No. employees 2005	419	2705.22	20,010.84	1	384,723
No. employees 2004	419	3118.29	20,606.48	1	362,063
No. employees 2003	419	2693.39	19,104.49	1	365,571

Note: Revenue data are given in Mio. Euro and is measured in prices of 2000 (GDP market price deflator). Revenues give the value of annual sales of goods and services—including other types of revenue such as dividends, interest, and rent—of the respective company.

following, we employ a regression analysis in order to test whether the relative allocation (as measured by the allocation factor) had an impact on competitiveness as measured by firm revenues—here: representing the ''ability to sell'' as one concept of competitiveness[4]—and employment of the German firm sample. The related correlations are shown in Table A2. As dependent variables we use the firm revenue change in 2005, i.e. revenue 2005 minus revenue 2004, as an indicator of their ability to sell, and firm employment change in 2005, i.e. number of employees 2005 minus number of employees 2004. As it is common for an analysis with cross-sectional firm data and a continuous dependent variable (in both cases), we use ordinary least squares (OLS) in a first step in order to compute our regression results. Still, as lined out in the previous sections, the explanatory variable of our special interest in this analysis, the allocation factor, may be endogenous in such setting. This is due to the fact that its calculation is based on (verified) emissions from 2005 given that historic emission data are not publicly available. However, if revenue and/or employment development in 2005 had an impact on the respective emissions, reverse causality would render our estimation results from OLS biased and inconsistent. As the most common technical solution in such setting, additionally to OLS, we make use of the instrumental variable technique employing the so-called two-stage least squares estimator (2SLS). Doing this, in the regression equation of our interest (second stage), the possibly endogenous allocation factor is replaced by its fitted values from its (first stage) regression on exogenous variables (so-called instruments). As instruments for the allocation factor, firm data on revenues and employment in differences and levels are available besides sectoral variables that partly more strongly correlate with the allowance factor than the economic variables do. Furthermore, OLS results have been controlled for possibly outlier-driven results using so-called iteratively reweighted least squares (IRLS), the most common ''robust'' regression method. However, analogously to the OLS results, IRLS regressions may suffer from reverse causality problems. Given this fact (and the fact that our central results on relative allocation are consistent for all techniques applied), IRLS results are only displayed in Appendix A, and interpretation focuses on OLS and 2SLS.

4.2. Estimation results

The central results of the regression analysis are shown in Table 2 (Tables A3 and A4 give more detailed results). In the regression analysis (1) using revenue changes between 2005 and 2004 as a dependent variable, we include, besides the sectoral indicator variables and a constant term, the allocation factor as the explanatory variable of our major interest, as well as revenue differences 2004ffi2003, revenue differences 2003ffi2002, revenues 2003, the number of employees 2003, and the differences of the number of employees 2004ffi2003 as explanatory (control) variables. Using lagged levels and differences of revenues and employment as explanatory variables, we circumvent possible reverse causality or simultaneity (endogeneity) problems that can arise if the dependent variable has an influence on these explanatory variables.[5] Regression (3) gives the respective 2SLS results,

[4] As our empirical assessment focuses on the EU ETS, we refer to within-EU competitiveness among EU firms here (as opposed to international competitiveness vis-à-vis non-EU regions).

[5] In contrast to the allocation factor that we instrument in the 2SLS approach, these explanatory variables are incorporated in lagged form instead of being instrumented. Thereby we assume actual lagged relationships between the explanatory and independent variables.

Table 2
Selected regression results for German firm sample

Dep. var.	Revenues 2005ffi2004 (Mio. Euro)		No. employees 2005ffi2004	
Regression number/ estimation technique	(1) OLS	(3) 2SLS	(7) OLS	(9) 2SLS
Allocation factor	122.14 (110.72)	50,538.58 (42,836.84)	30.23 (119.04)	ffi44,067.80 (48,347.34)
No. obs.	419	419	419	419
R^2	0.84	0.84	0.85	0.85
F-test (p-val.)	0.00	0.00	0.00	0.00

Note: (White) robust std. errors in brackets. Results from regressions including the full set of control variables. Detailed results including parameter estimates of the control variable set (cp. regression numbers), as well as results of regression equations from which insignificant control variables have been eliminated and the IRLS estimations are given in Appendix A.

and (5) the IRLS results. From (2), (4), and (6), insignificant explanatory variables (besides the allocation factor) have been eliminated. For OLS and 2SLS, such elimination of insignificant explanatory variables is supported by an F-test. All in all, our results show a good fit of the econometric model, with an R^2 of 84%. According to the results of the F-test, the null hypothesis of joint insignificance of all explanatory variables can be rejected at the 1% level for any equation. The results, i.e. the parameter estimates of significant variables as well as their significance levels, are relatively robust to the choice of estimation technique as well as to the elimination of insignificant explanatory variables.

The main insight of this regression is that we do not find empirical evidence for a significant impact of the relative allocation of EU emissions allowances on firm revenue development in 2005. From a theoretical emissions-market perspective, a higher relative (grandfathered) allowance allocation induces lower compliance costs of emissions regulation (see e.g. Böhringer et al., 2005). Thus, relative allowance allocation and the subsequent trading of emissions permits affect the cash flow of the regulated firms. Clearly, the impacts of environmental regulation on firm revenues, production and employment are more complex and depend on the allocation rule (Demailly and Quirion, 2007). Our estimation results show a positive coefficient of the allocation factor both in the OLS and 2SLS regressions, which, given large standard errors, does not significantly differ from 0 in all equations presented in Table A3. Our results thus suggest that companies that received a relatively high amount of allowances within the allocation process could not, consequently, increase their revenues compared to other German companies within the emissions trading scheme. Besides the sectoral indicator variables that show a highly significant impact on revenue development, which can, for example, be explained by differences in sectoral demand, most other control variables do not show significance at any conventional level. An exception to this is the coefficient of the number of employees in 2003 that enters with a negative sign in the equation (with an estimated coefficient of about ffi0.20), suggesting that firms with a larger working force were less successful in increasing their revenues in 2005. IRLS gives partly different results, but, as indicated above, does not show any significance for the estimated coefficient of the allocation factor either.

In regression analysis (7) using the changes in the number of employees between 2005 and 2004 as a dependent variable, we include, besides the sectoral indicator variables and a constant term, the allocation factor as the explanatory variable of our major interest, as well as revenues 2004–2003, revenues 2003, the number of employees 2003, the number of employees 2004–2003, and the number of employees 2003–2002 as explanatory (control) variables. As in regression (1)–(6), the use of lagged levels and differences of revenues and employment as explanatory variables is due to the potential problem of endogeneity as well as of assumed lagged relationships (see footnote 4). Here as well, the results are robust to the elimination of insignificant explanatory variables and show a good fit of the econometric model, with an R^2 even slightly higher than in regression (1)–(4) (up to 85%). According to the results of the F-test, the null hypothesis of joint insignificance of all explanatory variables can be rejected at the 1% level for all approaches used. Regression (9) gives the respective 2SLS results, (11) the IRLS results. From (8), (10), and (12), insignificant explanatory variables (besides the allocation factor) have been eliminated (exclusion is supported by the F-test for the 2SLS and IRLS cases).

Analogously to the revenue analysis, we did not receive empirical evidence for a significant impact of the relative allocation of EU emissions allowances on the change in (firm level) employment in 2005. For regression (7) (as well as (11) and (12)), the estimated coefficient of the allocation factor is positive. However, the magnitude of the coefficient is small and is not significant at any conventional level. According to economic theory, stringent environmental regulation may induce employment losses if the output effect of regulation (i.e. lower production and employment levels) dominates the substitution effect (i.e. the shift to a higher labour intensity of production). Our estimation results suggest, however, that firms with a lower allocation factor within the trading scheme did not react with worker layoffs on a net basis.

In regressions (8)–(10), the sign of the estimated allocation factor coefficient changes (for IRLS, again, it is positive in both regressions). However, the coefficients fail to show significance at any conventional level in all equations. In contrast to (1)–(6), the estimated coefficient of the number of employees 2004–2003 is—with a value of about 1—very high and negative. The coefficient, significant at the 1% level in each regression, suggests that the lay-off of workers in 2004 had a (similar) negative effect on the change of employment in 2005, i.e. the lay-off of one worker in 2004 resulted in the lay-off of an additional worker in 2005. This may be due to labour market rigidities as well as employment policies of the companies analyzed, provided that suspensions were relatively stable over time (2004–2005). Sectoral indicator variables have a highly significant impact on employment only using OLS. These results are therefore not very robust over the different econometric specifications, indicating that sectoral affiliation did not necessarily play a role in employment changes in 2005. Furthermore, the interpretation of the individual sectoral dummies is difficult, as the estimated parameters attribute the deviations of employment changes of the relative sector to those firms that formed part of sectors that were not explicitly modelled. Most other control variables fail to show significance at any conventional level. The effect of revenue as well as of employment development in 2004 on employment development in 2005 is extremely robust with regard to both point estimates and statistical significance. This undermines the findings of a positive relationship between 2004 revenue development and 2005 employment, as well as of a negative relationship between employment development in 2004 and 2005. At least as far as signs and significance of the estimated parameters are concerned, IRLS results resemble 2SLS.

5. Conclusions

This paper empirically investigates the role of the EU Emissions Trading Scheme (EU ETS) for competitiveness and employment at the firm level. We provide an overview over relative allowance allocation within the EU ETS, as well as an econometric analysis for a large sample of German ETS firms in order to assess the economic impacts associated with emissions allocation under the EU ETS.

Our calculations suggest that the total EU ETS was generally long in 2005. The long position is very large in Lithuania, while other countries were short in emissions allowances. Regarding the competitiveness effects of EU environmental regulation, we conduct an econometric ex-post regression analysis for Germany, which, to our knowledge, is the first of its kind concerning the EU ETS. Following the competitiveness concept ''ability to sell'', as an empirical indicator for competitiveness we employ firm revenues. As a second economic indicator we use employment levels of the respective firms. Our econometric analysis provides evidence on the fact that the allowance allocation within the EU ETS framework did not have a significant impact on revenues and employment of regulated German firms. Our results thus suggest that for regulated companies the competitiveness impacts of the emissions allocation within the first phase of the EU ETS were not pronounced. This finding could be due to the low overall burden of emissions regulation within the EU ETS.

Some disclaimers apply to these results. First, it is definitely very early to conduct an ex-post analysis for the EU ETS. In this respect, it is possible that competitiveness effects of this regulation could occur after 2005. Consistent economic firm data at a European level for 2005 or later were not available to us, so that our—first, and, due to the small dataset, basic—econometric analysis could only be performed within a case study for Germany, the most important country within the EU ETS according to the verified emissions. Furthermore, ex-post analyses do not have to be restricted to revenues and employment, although these are definitely two factors of great interest in the context of environmental regulation. Other measures of interest may be, e.g., innovation, profits, and international trade effects that could not be tackled within the analysis conducted here. All in all, future empirical research in many directions is needed to complement these first ex-post insights into the effects of regulation according to the EU ETS.

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Appendix A. Data on EU ETS allocation

Our analysis is based on data on approximately 12,000 installations being covered by the EU ETS legislation. Each installation has an Operator Holding Account in its national registry to which the allowances are submitted, and each member state of the European Union has an obligation to interlink the national registry with the

Table A1
Sectoral distribution of sample firms

Sector	Frequency: no. sample firms (%)
Mining	9 (2)
Electricity	55 (13)
Energy	29 (7)
Business	20 (5)
Pulp & paper	43 (10)
Coke & petroleum	8 (2)
Other manufacturing	102 (24)
Other	153 (37)
Total	419 (100)

Table A2
Correlation analysis for German firm sample

	Alloca-tion factor	Revenues 2005ffi2004	Revenues 2004ffi2003	Revenues 2005	No. employees 2005ffi2004	No. employees 2004ffi2003	No. employees 2005	Mining	Electricity	Energy	Business	Pulp & paper	Coke & petroleum	Other manufacturing
Allocation factor	1.00													
Revenues 2005ffi2004	0.01	1.00												
Revenues 2004ffi2003	ffi0.03	ffi0.36	1.00											
Revenues 2005	ffi0.02	ffi0.52	0.57	1.00										
No. employees 2005ffi2004	0.03	ffi0.22	ffi0.03	0.89	1.00									
No. employees 2004–2003	ffi0.02	0.06	0.43	0.35	0.19	1.00								
No. employees 2005	ffi0.01	ffi0.90	0.56	0.79	0.02	0.02	1.00							
Mining	0.03	0.02	0.01	0.04	0.03	0.03	ffi0.02	1.00						
Electricity	0.17	0.02	ffi0.06	ffi0.06	0.03	0.03	ffi0.04	ffi0.13	1.00					
Energy	0.02	0.01	ffi0.04	ffi0.04	0.02	0.02	ffi0.03	ffi0.09	ffi0.11	1.00				
Business	ffi0.03	ffi0.03	ffi0.03	ffi0.04	0.01	0.01	ffi0.02	ffi0.07	ffi0.09	ffi0.06	1.00			
Pulp & paper	ffi0.05	0.01	ffi0.04	ffi0.06	0.03	ffi0.03	ffi0.04	ffi0.05	ffi0.13	ffi0.09	ffi0.08	1.00		
Coke & petrol.	ffi0.04	ffi0.01	ffi0.02	ffi0.01	0.01	ffi0.01	ffi0.02	ffi0.02	ffi0.05	ffi0.04	ffi0.03	y0.05	1.00	
Other manuf.	ffi0.04	0.04	ffi0.04	ffi0.05	0.03	ffi0.05	ffi0.04	ffi0.08	ffi0.22	ffi0.15	ffi0.12	ffi0.19	ffi0.08	1.00

Note: 419 observations. Pearson's correlation coefficients for the respective variable pairs are given. Lagged levels and differences of higher order for the revenue and employment variables are omitted for brevity.

EU-wide databank EU Community Transaction Log. The Community Transaction Log's web pages contain information on allowances that have been allocated in accordance with the final National Allocation Plans, verified emissions, surrendered allowances and compliance status for all installations in member states with registries. We assessed the emissions data from the Community Transaction Log in two steps:

ffldata extraction from the Community Transaction Log and data processing; and
fflaggregation of installation-level data on the sectoral and national levels.

A.1. The AMADEUS database

Besides the emissions data from the Community Transaction Log, economic data are of great importance for assessing the competitiveness effects of the EU ETS. AMADEUS (Analyse Major Databases from European Sources) is a comprehensive, pan-European database containing economic and financial information on 9 million public and private companies. AMADEUS combines data from over 30 specialist sources and provides data in a comparable format. It is created and produced by Bureau van Dijk. In this analysis, sectoral information for our German firm sample is based on the four-digit NACE (industry) codes of the firms provided by AMADEUS. According to this, we have created several indicator variables that are given the value 1 for a company that forms part of the respective industry, and 0 otherwise. The indicator variables are ''electricity'' (13% of the sample firms; NACE code between 4000 and 4020, ''production and distribution of electricity''), ''energy'' (7% of the sample firms; NACE code between 4020 and 4500, ''manufacture of gas; distribution of gaseous fuels through mains'', ''steam and hot water supply'', ''collection, purification and distribution of water''), ''pulp & paper'' (10% of the sample firms; NACE code between 2100 and 2200, industry subsection ''manufacture of pulp, paper, and paper products''), ''mining'' (10% of the sample firms; NACE code between 1000 and 1500, industry subsection ''mining and quarrying''), ''coke & petroleum'' (2% of the sample firms; NACE code between 2300 and 2400, ''manufacture of coke, refined petroleum products and nuclear fuel''), ''other manufacturing'' (24% of the sample firms; NACE code between 2600 and 3700, manufacture of

Table A3
Regression results on 2005 revenue development for German firm sample

Dep. var.	(1) OLS	(2) OLS	(3) 2SLS	(4) 2SLS	(5) IRLS	(6) IRLS
	Revenues 2005ffi2004 (Mio. Euro)	Revenues 2005ffi2004 (Mio. Euro)	Revenues 2005ffi2004 (Mio. Euro)	Revenues 2005ffi2004 (Mio. Euro)	Revenues 2005ffi2004 (Mio. Euro)	Revenues 2005ffi2004 (Mio. Euro)
Allocation factor	122.14 (110.72)	83.72 (117.58)	50,538.58 (42,836.84)	1552.76 (2140.87)	ffi0.33 (0.75)	ffi0.36 (0.76)
Revenues 2004ffi2003 (Mio. Euro)	ffi0.03 (0.66)	–	0.01 (1.25)	–	0.22*** (0.00)	0.21*** (0.00)
Revenues 2003ffi2002 (Mio. Euro)	0.01 (0.18)	–	ffi0.30 (0.84)	–	ffi0.02*** (0.00)	ffi0.02*** (0.00)
Revenues 2003 (Mio. Euro)	ffi0.05 (0.18)	–	1.24 (1.72)	–	0.03*** (0.00)	0.03*** (0.00)
No. employees 2003	ffi0.18*** (0.07)	ffi0.19*** (0.03)	ffi0.66 (0.64)	ffi0.19*** (0.03)	0.00*** (0.00)	–
No. employees 2004–2003	0.15 (0.11)	–	0.34** (0.14)	–	0.00*** (0.00)	–
Mining	ffi225.89 (255.96)	–	338.81 (6456.00)	–	6.21** (2.99)	5.58* (2.95)
Electricity	ffi483.89*** (134.43)	ffi471.20*** (112.97)	ffi8980.94 (11737.64)	–	7.61*** (1.39)	7.07*** (1.30)
Energy	ffi491.44*** (128.93)	ffi488.11*** (105.70)	ffi3335.58 (5964.47)	ffi443.98** (212.50)	4.92*** (1.76)	4.17** (1.70)
Business	ffi1083.73** (529.95)	ffi1085.56* (598.66)	5823.88 (6566.77)	–	1.28 (2.08)	–
Pulp & paper	ffi482.81*** (116.57)	ffi478.65*** (100.03)	2020.76 (3730.35)	ffi293.81* (155.56)	0.39 (1.51)	–
Coke & petroleum	ffi836.93* (479.15)	ffi878.94* (469.98)	5482.63 (7324.90)	–	7.61** (3.15)	8.07*** (3.15)
Other manufacturing	ffi181.27 (192.49)	–	423.98 (3146.08)	–	1.38 (1.12)	–
Constant term	436.91*** (152.38)	475.14*** (164.28)	ffi61,101.61 (51,916.09)	ffi1431.85 (2611.28)	ffi3.17*** (1.18)	ffi2.44** (1.05)
No. obs.	419	419	419	419	413	414
R^2	0.84	0.80	0.84	0.82	–	–
F-test (p-val.)	0.00	0.00	0.00	0.00	0.00	0.00
F-test on excl. exp. var. (p-val.)	–	0.50	–	0.29	–	0.00

Note: Std. errors in brackets (OLS, 2SLS: white robust std. errors). *, ** and *** show significance at the 10%, 5%, and 1% levels, respectively.

Table A4
Regression results on 2005 employment development for German firm sample

Dep. var.	(7) OLS	(8) OLS	(9) 2SLS	(10) 2SLS	(11) IRLS	(12) IRLS
	No. employees 2005ffi2004	No. employees 2005ffi2004	No. employees 2005ffi2004	No. employees 2005ffi2004	No. employees 2005ffi2004	No. employees 2005ffi2004
Allocation factor	30.23 (119.04)	ffi12.00 (62.79)	ffi44,067.80 (48347.34)	ffi8126.44 (9355.91)	0.49 (2.04)	0.40 (2.21)
Revenues 2004ffi2003 (Mio. Euro)	4.58*** (0.71)	4.29*** (0.70)	4.43*** (0.93)	4.38*** (0.62)	0.02*** (0.01)	0.02*** (0.01)
Revenues 2003 (Mio. Euro)	0.05 (0.28)	–	ffi0.71 (1.38)	–	0.00** (0.00)	–
No. employees 2003	ffi0.05 (0.11)	–	0.22 (0.52)	–	0.00*** (0.00)	ffi0.02*** (0.00)
No. employees 2004ffi2003	ffi1.00*** (0.04)	ffi1.01*** (0.04)	ffi1.15*** (0.16)	ffi1.05*** (0.05)	ffi1.00*** (0.00)	ffi1.01*** (0.00)
No. employees 2003ffi2002	ffi0.85 (0.80)	–	ffi1.40 (1.98)	–	0.00 (0.00)	–
Mining	486.99 (316.47)	373.76* (193.43)	323.98 (5404.09)	–	ffi4.86 (8.01)	–
Electricity	562.16* (307.10)	431.50** (183.63)	8032.52 (11732.76)	–	0.62 (3.76)	–
Energy	576.89** (280.85)	460.36*** (178.56)	3036.61 (5619.92)	–	9.57** (4.77)	–
Business	555.29* (315.56)	394.59* (228.14)	ffi4859.04 (6423.88)	–	6.85 (5.62)	–
Pulp & paper	565.95* (320.64)	432.77** (186.05)	ffi1540.41 (3394.63)	–	ffi0.62 (4.07)	–
Coke & petroleum	624.19** (273.35)	512.96*** (192.22)	6473.68 (7243.06)	–	11.07 (8.56)	–
Other manufacturing	285.82 (365.37)	–	ffi235.53 (2741.93)	–	4.31 (3.02)	–
Constant term	ffi642.96* (388.52)	ffi467.25** (198.56)	53,153.02 (58,410.43)	9668.14 (11,335.02)	ffi3.32 (3.17)	4.37 (3.03)
No. obs.	419	419	419	419	415	416
R^2	0.85	0.83	0.85	0.35	–	–
F-test (p-val.)	0.00	0.00	0.00	0.00	0.00	0.00
F-test on excl. exp. var. (p-val.)	–	0.00	–	0.96	–	0.21

Note: Std. errors in brackets (OLS, 2SLS: white robust std. errors). *, ** and *** show significance at the 10%, 5%, and 1% levels, respectively.

non-metallic mineral products, basic metals and fabricated metal products, machinery and equipment, electrical and optical equipment, transport equipment, other manufacturing), and ''business'' (5% of the sample firms; NACE code between 7000 and 7500, section ''real estate, renting, and business activities'').

A.2. The CREDITREFORM database

This database is a financial and economic database that includes information of sales and employment of German firms. It is the most comprehensive database on German firms, containing a random sample of 20,000 solvent and 1000 insolvent firms in Germany. Given a consistent form identification number, it is coherent with the AMADEUS database. From the CREDITREFORM database we use levels and differences from firm revenue and employment data between 2002 and 2005; from AMADEUS we use generated sectoral indicator variables (see above). Those data have been matched with the allocation factor (allowances allocated divided by verified emissions) from the Community Transaction Log. This has been conducted by supplementing allocation data that have been aggregated at the firm level with AMADEUS and CREDITREFORM data. The main criteria for this database matching were the respective company names and addresses. The matching results have been carefully checked for consistency reasons (Tables A1–A4).

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Environmental regulation and labor demand: evidence from the South Coast Air Basin

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Abstract

The devolved nature of environmental regulation generates rich regulatory variation across regions, industries and time. We exploit this variation, using direct measures of regulation and plant data, to estimate employment effects of sharply increased air quality regulation in Los Angeles. Regulations were accompanied by large reductions in NO_x emissions and induced large abatement investments for re®neries. Nevertheless, *we ®nd no evidence that local air quality regulation substantially reduced employment*, even when allowing for induced plant exit and dissuaded plant entry. Regulations affected employment only slightly Ð partly because regulated plants are in capital and not labor-intensive industries. These ®ndings are robust to the choice of comparison regions.

Keywords: Environmental regulations; Employment; Air quality; Effects

JEL classi®cation: J0; J4; L5; L6

1. Introduction

The increasing cost of environmental regulation[1] in the past 25 years has fueled

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[1]American manufacturing plants invested $4.3B in 1994 to abate air pollution (4% of capital investment) and incurred another $6.1B in air pollution abatement operating costs (United States Bureau of Census, 1996). The EPA estimates the cost of abatement for the US at 2.1% of GDP for 1990 (Jaffe et al., 1995).

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a debate over its cost-effectiveness in improving environmental quality, and the tightening of national ambient air quality standards in 1997 has intensi®ed that debate. Chief among the perceived costs of regulation is the loss of employment, an issue that looms large in policy debates on environmental regulation.[2] Fears of an inter-regional `race to the bottom' in setting lax environmental regulations to avoid local job loss was one reason for the establishment of the US Environmental Protection Agency (EPA). In light of such concerns, ef®cient (and politically feasible) regulation requires precise estimates of its effects on employment.

Environmental regulation does not necessarily reduce labor demand. While abatement activities probably increase marginal costs and decrease labor demand through reductions in sales, abatement activities may, in fact, complement labor Ð leading to an increase in labor demand. Theory alone yields an ambiguous prediction of the over-all employment effects of environmental regulation. Existing empirical studies have likewise yielded mixed results on these employment effects (Jaffe et al., 1995).[3]

Estimating the effects of environmental regulation is dif®cult for a number of reasons. Some studies have estimated the effects of regulation by regressing outcomes on measured abatement activity (see for example, Gray and Shadbegian (1993b). This approach is confounded by selection bias and measurement error. Plants that can abate at low cost are likely to have the smallest employment effects and are most likely to abate voluntarily Ð without the impetus of regulation. This selection effect will bias estimates of the effects of *induced* abatement on employment, making abatement appear less costly than it actually is. Measurement error in abatement costs also is likely to bias estimated effects toward zero because of attenuation bias.

Our solution to these estimation problems is to gather detailed micro data on local air pollution regulations in a speci®c region of the country and to construct relevant treatment and comparison groups for each industry affected by the local air quality regulations that we study. Comparison groups are constructed to represent the counterfactual in which treated plants are not subject to local air pollution regulation. We code regulations as binary indicators and estimate the

[2] For example, in California, employment effects must be taken into account in the formulation of environmental regulations (Sept. 1994, resolution 94-36, South Coast Air Quality Management District).

[3] A number of empirical studies have investigated the effect of *federal* regulation on employment and other economic outcomes in manufacturing. Gray (1987) studies the relationship between enforcement and compliance for EPA and Occupational Safety and Health Act regulation, ®nding that compliance is higher for industries with high pro®ts, high wages, low compliance costs, and frequent inspections. Bartel and Thomas (1987) estimate the effect of EPA and OSHA on both wages and pro®ts, ®nding *regional* differences in the effect of regulation. Gray and Shadbegian (1993a,b) ®nd that manufacturing plants with high abatement costs have high labor demand. Other studies analyze effects of a particular set of environmental regulations on a *speci®c* industry. For example, Hartman et al. (1979) ®nd that federal environmental regulation reduces employment in the U.S. copper industry.

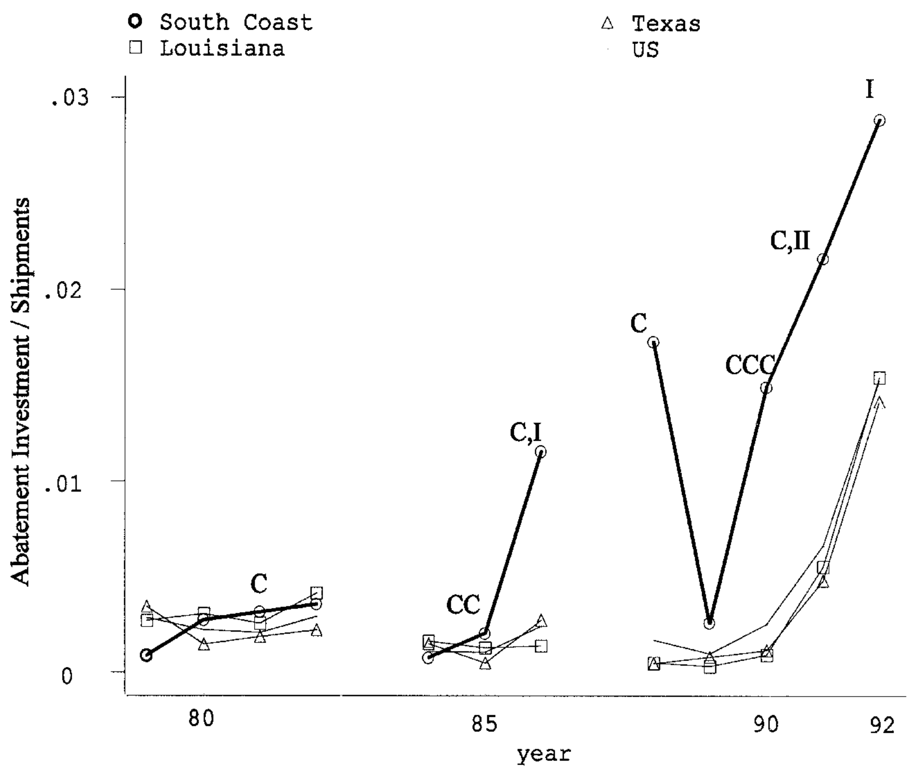
effect of regulation on employment directly (rather than the effect of abatement expenditures on employment).

The richness of our data comes from the structure of US environmental regulation. Since the EPA delegates much regulatory authority to state and local agencies, regulatory stringency varies across regions for the same industries, depending upon local environmental quality.We focus on the manufacturing sector in this paper. Our innovation is in directly estimating the effects of local air pollution regulations using a quantitative approach that includes comparison plants in the same precisely de®ned industry. This allows us to check the robustness of our results by alternating the regions used for comparison plants. To implement this approach we quantify local air pollution regulations as binary covariates, a lengthy procedure that involves numerous subjective judgements. Our principal methodological contribution is a coding procedure that avoids bias due to `data mining' using a simple method we call `sequestering the data.'

The Los Angeles area provides our study with an episode of sharp increase in local air quality regulation in the 1980s. These local regulations apply *over and above* federal and state regulations. The South Coast Air Quality Management District (SCAQMD), which regulates the air basin containing Los Angeles and her suburbs,[4] has enacted some of the country's most stringent air quality regulations since 1979. These were triggered by the interaction of increasingly stringent air quality standards and abysmal air quality in the South Coast. Poor air quality is due both to emissions and topographical conditions: the unique climate and geography of the region contribute to a thermal inversion, which traps pollutants near ground level. Thus the SCAQMD found itself far out of compliance with the 1970 EPA national ambient air quality standards. It responded by the late 1970s, adopting a set of extremely stringent regulations in an attempt to meet those standards, an effort primarily aimed at reducing emissions of nitrous oxides (NO_x). For example, Fig. 1 illustrates the costs imposed by these regulations on South Coast oil re®neries, the most highly regulated of manufacturing industries. Beginning in 1986, when these regulations came into effect, South Coast re®neries faced much higher abatement investment costs than did re®neries in Texas and Louisiana, regions with less stringent state regulations and no local air quality regulation.[5] The strict and sometimes innovative approach to environmental regulation in the South Coast has been copied by other regions in their attempts to comply with the national ambient air quality standards. The increased stringency of 1997 EPA air quality standards may eventually force adoption of similar regulations in other regions, so estimated employment effects of South Coast regulations should be of interest to regulators elsewhere in the country.

[4]The South Coast Air Basin consists of Los Angeles County, Orange County, Riverside County, and the non-desert portion of San Bernardino County.

[5]Some state regulations may be location speci®c.When these location-speci®c regulations exist, they target non-compliance regions within the state. See footnote 15 for details.



Investment/Value of Shipments in Refineries

Fig. 1. Abatement investment/value of shipments in re®neries. Source: PACE survey. Notes: The graph compares air pollution abatement investment in oil re®neries in the South Coast region to that in the re®neries of Texas, Louisiana and the entire US. Abatement investment is calculated from the PACE survey. Each compliance date for a South Coast regulation is labeled with a `C' and each date of increased stringency is labeled with an `I'. For instance, in 1991 one regulation had a compliance date and two had dates of increased stringency. Abatement investment data are unavailable in 1983 and 1987.

We exploit three dimensions of variation Ð across regions, industries, and time Ð to estimate the effects of local air quality regulation on labor demand, constructing a sample including both plants in the South Coast subject to changes in local regulation and plants in the same industries in other regions of the US (the comparison plants). Plants in the comparison regions are subject to federal and state regulations, but generally do not face additional local regulations as well. The stringency of federal regulations depends on whether the region is in attainment of national ambient air quality standards. For ozone, considered by many to be the most serious of the criteria air pollutants, the South Coast is out of attainment during the entire period studied. Most of the regulatory pressure on South Coast plants is from local regulation, which are more stringent than the federal regulations. Our estimates are of the effect of local South Coast regulations in contrast to the average level of local regulation in the comparison regions. We report separate estimates using attainment and non-attainment comparison regions, as well as a Texas±Louisiana comparison region, which has a similar industrial structure to the South Coast but less stringent air pollution regulation.

To match the degree of detail in regulatory variation we use two panels of plant level data made available to us by special arrangement with the Census Bureau: the Pollution Abatement Costs and Expenditures Survey (PACE) in 1979±1991 and the Census of Manufactures in 1977, 1982, 1987 and 1992. These data allow us to identify plants subject to new South Coast regulations and to compare them with plants (plant-years to be precise) not subject to new regulations. Using this approach we can remove potentially confounding plant effects, and industry and/or region speci®c shifts in employment in estimating the effect of regulatory change on employment. In an analysis of the Los Angeles area during the 1980s these are key issues, as the regional concentration of declining defense industries led to a secular decline in employment which we argue has been falsely attributed to environmental regulation. We claim that the incidence of regulation is orthogonal to sample selection because the timing of regulation was due to the con¯uence of the stringency of federal (EPA) air quality standards and the serious air quality problem in Los Angeles.

We ®nd that while regulations do impose large costs they have a limited effect on employment. Compliance with a new regulation induces \$0.5M of abatement investment per affected plant (with a standard error of \$0.2M). Increases in stringency of an existing regulation induce \$1.9M (\$1M) of abatement investment. The employment effects of both compliance and increased stringency are fairly precisely estimated zeros, even when exit and dissuaded entry effects are included. Point estimates of the *cumulative* effect of 12 years of air quality regulation from 1979±1991 vary according to the comparison regions used, from 2600 to 5400 jobs created, with standard errors about the size of the estimates. Point estimates based on the quintennial Census (which allow for entry and exit of plants, long term response and include 1992 regulations as well) vary more with comparison groups, from 9600 jobs lost to 12 300 jobs gained. These are very small effects in a region with 14 million residents and about 1 million manufacturing jobs. The large negative employment effects alluded to in the public debate (Goodstein, 1996) can clearly be ruled out.

Small employment effects are probably due to the combination of three factors: (a) regulations apply disproportionately to capital-intensive plants with relatively little employment; (b) these plants sell to local markets where competitors are subject to the same regulations, so that regulations do not decrease sales very much; and (c) abatement inputs complement employment.

This paper is similar in spirit to investigations of how plant location responds to differences in *local* environmental regulations. Henderson (1996), Becker and Henderson (in press), and Greenstone (1999) use as a proxy for local regulatory activity an indicator for whether a county attains compliance with federal standards. They both ®nd that transition into attainment is associated with an incursion of polluting plants. Greenstone also ®nds negative employment, investment and output effects for continuing plants. Gray (1997) ®nds that states with more stringent enforcement have fewer plant openings. Levinson (1996) examines plants in pollution intensive industries, ®nding little impact of regulation on the location of new manufacturing plants (1982±1987).

This paper is related in method to a recent literature in labor economics and public ®nance that uses cross-sectional variation in changes in regulations, laws and institutions to study the effects of these changes. The variation is often arguably exogenous and the results are of interest to policy makers contemplating similar regulatory changes. Meyer (1995) provides a survey. We offer two innovations to that literature: ®rst, we demonstrate that useful regulatory variation can come from a set of diverse, technical regulations once they are appropriately quanti®ed. Second, we show that geographical variation observed within industry in plant data allows the use of comparison plants in different regions to test the robustness of the estimates.

Several characteristics of local air quality regulation programs make our approach an attractive alternative to existing evaluation methods. Air quality regulation is too expensive to allow random assignment of treatment. Similar to the job training programs discussed by Hotz et al. (1998), local air quality regulation efforts involve a mixture of components applied to a population with distinct characteristics. In these situations Hotz et al. (1998) stress the need for precise measurement of the characteristics of program components and of the treated population to allow prediction of a program's effects on other populations. It is hard to imagine an approach not based on micro regulations and plant level data that satis®es the two critical conditions for credible estimation: (a) enough detailed information on industry and regional characteristics to remain unconfounded by secular trends, and (b) enough comparison regions so that there is suf®cient overlap in characteristics between treatment and comparison groups to allow estimation.

The paper proceeds as follows. Section 2 provides background about environmental regulation in the SCAQMD. In Section 3, we derive estimating equations from a model of labor demand under regulation. Section 4 describes the data. In Section 5, we present results and Section 6 concludes.

2. Background: the regulation of air quality

An important aspect of the EPA's mission is to set national standards for environmental quality, to forestall a `race to the bottom' among regions attempting to entice industries to locate in regions with more lax environmental standards.

The national standards are based on health criteria alone, not on economic cost-bene®t analyses. For air pollution, these national ambient air quality standards (NAAQS) apply to six `criteria' air pollutants (sulfurous oxides, nitrous oxides, particulate matter, volatile organic compounds, ozone, and airborne lead). States are responsible for state implementation plans (SIPs) which the EPA must approve. The plan indicates how the state will ensure that all its regions attain the standards. The EPA can withhold federal funds from states without approved SIPs and has threatened to take over environmental regulation in California if the state does not comply with the NAAQS.

Federal environmental regulation of stationary sources is generally limited to new sources of pollution (New Source Performance Standards, `NSPS'), except in `non-attainment' regions that do not meet the federal standards and in regions deemed `pristine' (Prevention of Signi®cant Deterioration regions, or `PSD'). In non-attainment regions all new investment must meet the `lowest achievable emissions rate' standard. In pristine regions new investment must meet the less severe `best available control technology' standard. Both the `lowest achievable' and `best available' standards are more demanding than the NSPS. New sources of pollution and major modi®cations to existing sources are restricted in both regions. All other sources of pollution, including existing stationary sources and mobile sources generally are regulated at the state level.

In California, air pollution from mobile sources is regulated by the California Air Resource Board, while the regulation of stationary sources is delegated to 34 local air quality management districts. The South Coast Air Quality Management District (SCAQMD) is responsible for the South Coast Air Basin in the area around Los Angeles.[6] The South Coast is further from attainment of the NAAQS than any other large region, hence the unprecedented severity of regulations which came into force in the mid-1980s.

Severe air pollution in the Basin is partly due to weather patterns. The Basin is arid, with little wind, abundant sunshine, and poor natural ventilation Ð conditions that exacerbate air pollution, especially the formation of ground level ozone.[7] It is densely populated with high concentrations of motor vehicles and industry. In 1990, the Basin contained 4% of the US population and 47% of the population of California.

When the NAAQS were ®rst established, the Basin was out of attainment for four of the six criteria pollutants. Hall et al. (1989) report that non-attainment of federal standards between 1984 and 1986 increased the death rate by one in ten thousand (a risk that doubles in San Bernardino and Riverside Counties).[8] Over

[6] In 1977, Orange, Riverside, and the non desert portion of San Bernardino Counties joined the Los Angeles County Air Pollution Board to form the SCAQMD.

[7] Ozone is produced by a combination of volatile organic compounds, NO_x and sunlight.

[8] For comparison, the risk of death from an automobile accident in California is 2/10 000.

half the Basin's population experienced a stage 1 ozone alert annually, during which children were not allowed to play outdoors. The average resident suffered 16 days of minor eye irritations and 1 day on which normal activities were substantially restricted.

The South Coast responded with local air quality regulations, over and above those imposed by the EPA and the State. These included heavy regulation of industrial emissions, generally mandating emission reductions and investment in emission control equipment. Table 1 illustrates the associated increase in abatement costs. Between 1979 and 1991 South Coast manufacturing plants increased air pollution abatement costs by 138%, nearly twice the national rate of increase, and increased air pollution abatement investment by 127%, *ten times* the national rate of increase. South Coast oil re®neries incurred the lion's share of increased abatement costs, accounting for the majority of abatement investment and operating costs by 1991.[9]

Fig. 1 illustrates the effect of these regulations on abatement investment in oil re®neries, where most measured abatement took place. The top line reports abatement investment as a proportion of shipments in South Coast re®neries, while the other three report that proportion in the re®neries of Texas, Louisiana and the entire US. Re®nery abatement investment is much higher than that in the comparison regions in 1986, 1988, 1990, 1991 and 1992. The letters `C' and `I' indicate a South Coast re®nery regulation with a compliance or an increased stringency date in that year, respectively. All years with a large gap between South Coast abatement and abatement in the comparison regions are years in which South Coast regulations came into force. Years in which abatement investment is similar in the South Coast and other regions are years without new South Coast regulations. Note that almost all of the regulations were associated with high abatement costs.

Table 1
Air pollution abatement control expenditures (Millions of 1991 Dollars)[a]

	Capital Expenditures		Operating Cost	
	South Coast	US	South Coast	US
1979	101	3313	125	2820
1991	229	3703	298	4978
% Growth				
1979±91	127	12	138	77

[a] Source: Authors' calculations from PACE micro data. Figures are slightly smaller than published totals for US Manufacturing.

[9] Berman and Bui (1998) provide a detailed description of abatement in re®neries.

Regulation signi®cantly improved ambient air quality. Between 1976 and 1993 the Basin reduced out-of-attainment days by 47%, from 279 to 147. The South Coast program emphasized decreasing NO_x emissions (primarily to reduce ozone, but also to reduce PM_{10} and because the federal NO_x standard was attainable). Fig. 2 illustrates the role of local, as opposed to state or federal, regulation in reducing NO_x emissions by manufacturing plants. It shows the share of South Coast plants in California's NO_x emissions in three categories of manufacturing: oil re®neries; other plants affected by local regulations in the South Coast; and plants not affected by South Coast regulations. The comparison to plants in the same industries in the rest of California allows a contrast with the effects of state and federal regulations over and above which the South Coast regulations apply. Oil re®neries in the South Coast show a steady decline in NO_x share, which accelerates after 1987. Non-re®neries covered by regulations (e.g. chemical, cement, and heavy manufacturing) show a reversal with an increasing share of

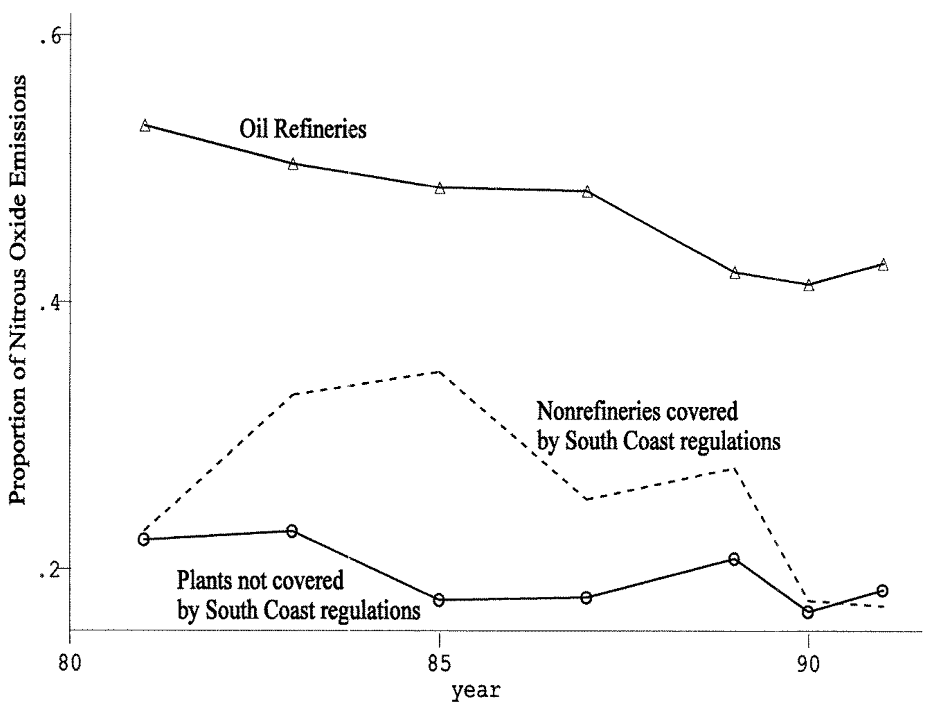


Fig. 2. Relative decline in South Coast NO_x emissions by regulatory category. Note: The ®gure describes the nitrous oxide emissions of South Coast manufacturing as a proportion of California's manufacturing in each of three categories: Oil re®neries (SIC 29); other industries subject to South Coast regulations at any time between 1979 and 1993; industries not subject to South Coast regulations at any time between 1979 and 1993.

state emissions after 1985.[10] Plants not covered by South Coast regulations reduced emissions no faster than did comparable plants in the rest of California. Despite this regulatory effort, in 1993 the South Coast remained out of compliance for three other criteria air pollutants (PM_{10}, ozone and CO) and still had the highest annual average NO_x level in the nation.

3. Labor demand under environmental regulation

In this section we motivate our estimating equations with a model of labor demand that allows regulation to act through two separate mechanisms, the output elasticity of labor demand and the marginal rates of technical substitution between abatement activity and labor. The partial static equilibrium model of production (Brown and Christensen, 1981) allows for the levels of some `quasi-®xed' factors to be ®xed by exogenous constraints, rather than by cost minimization alone. We apply that approach, treating costs incurred to comply with environmental regulation Ð pollution abatement capital investment and abatement costs (labor, materials and services) Ð as `quasi-®xed'. Labor, materials and productive (regular) capital are the variable factors.

Assume a cost-minimizing ®rm operating in perfectly competitive markets for inputs and output. There are J variable inputs and K `quasi-®xed' inputs. The variable cost function has the form:

$$CV = H(Y, P_1, \ldots, P_J, Z_1, \ldots, Z_K) \tag{1}$$

where Y is output, the P_j are prices of variable inputs, and Z_k are quantities of quasi-®xed inputs. Pro®t maximization implies the ®rst order conditions that yields demand for the variable input labor, L, as a function of output, quantities of the other inputs, and prices, which we approximate by the linear equation:[11]

$$L = \alpha + \rho_Y Y + \sum_{k=1}^{K} \beta_k Z_k + \sum_{j=1}^{J} \gamma_j P_j. \tag{2}$$

[10] In the period between 1983±85 and 1990±91 NO_x emissions declined by 15 000 tons (standard error = 6600) in South Coast re®neries and by 5400 tons (7300) in other California re®neries. NO_x emissions declined by 8300 tons (4200) in other regulated South Coast plants but increased by 11 900 tons (13 300) in plants in the same industries in the rest of California. Standard errors are calculated using raw data on point sources, corrected for heteroskedasticity, grouped plant effects and industry effects.

[11] We are restricted to a linear approximation by data constraints. For pollution abatement capital we observe only changes (investment), not levels.

The reduced form effect of regulation (R) on labor demand can be written:

$$L = \delta + \mu R. \tag{3}$$

The effects of regulation on employment are through the mechanism:

$$\frac{\mathrm{d}L}{\mathrm{d}R} = \rho_Y \frac{\mathrm{d}Y}{\mathrm{d}R} + \sum_{k=1}^{K} \beta_k \frac{\mathrm{d}Z_k}{\mathrm{d}R} + \sum_{j=1}^{J} \gamma_j \frac{\mathrm{d}P_j}{\mathrm{d}R} = \mu. \tag{4}$$

If input markets are large and competitive, regulatory change will have no effect on input prices so the final term in (4) will disappear, leaving the first two. The first term reflects the effect of regulation on demand for variable factors through its effect on output. This output effect of environmental regulation is widely believed to be negative (though theory gives no clear prediction: if compliance is achieved through an investment that reduces marginal costs, $\mathrm{d}Y/\mathrm{d}R$ could be positive). The second term reflects the effect of regulation on demand for variable factors through its effect on demand for quasi-fixed abatement activities, Z, and the marginal rates of technical substitution between abatement and variable factors. The change in demand for abatement activity due to an increase in regulation, $\mathrm{d}Z/\mathrm{d}R$, must be positive.

The signs of the β_k, which reflect whether abatement activity and labor are complements or substitutes, are not known a priori. Abatement technologies fall into two general categories, 'end-of-pipe' and 'changes in process.' End-of-pipe technologies such as scrubbers and precipitators, remove pollutants from *existing* discharge streams before their release into the environment, and probably complement labor, particularly production labor. Improvements in production process, such as the installation of more efficient boilers which operate at lower levels of emissions, often reduce demand for production workers due to a general skill-bias of technological change. Hence the signs of the β_k are ambiguous, which is the main reason that the sign of μ, the employment effect of regulation, cannot be predicted from theory alone.[12]

Some of the employment effects of regulation may be through induced exit of plants, as output is reduced to zero, and through dissuaded entry (Henderson, 1996; Levinson, 1996; Gray, 1997). For those effects only the output effect (the

[12] Ideally, we would estimate the parameters of (4) using regulatory change variables as instruments for Y (value added) and Z (the quasi-fixed factors). This proves to be too ambitious a demand to make of our data.

®rst term of (4)) is relevant, so the employment effect of regulation through induced exit and dissuaded entry is likely to be negative.

4. Data description

We exploit variation in regulation across industries, regions and time by using plant level data.We use two (unbalanced) panels drawn from Census Bureau data: The Survey of Pollution Abatement and Control Expenditures (PACE), linked to the Annual Survey of Manufactures (ASM), and the Census of Manufactures (CM). (Plant records from the ASM linked over time are the basis of the Longitudinal Research Database (LRD) panel compiled by the Center for Economic Studies of the Census Bureau).

The ASM samples the population of manufacturing plants, including large plants (250 or more employees) with certainty. Smaller plants are rotated out of the sample at 5-year intervals. From these data we use the employment, value added, and capital investment variables. PACE reports abatement investment and operating costs by the medium abated (air, water, and hazardous waste).We use air pollution abatement costs and investments. To account for entry and exit we use the Census of Manufactures, which covers the population of manufacturing plants every 5 years.[13] From these data we make use of employment, value added, and capital investment. These data are described fully in an appendix available from the authors.

Our most dif®cult task was the construction of measures of regulatory change. We constructed a data set for the Basin detailing *all* changes in local environmental regulation affecting manufacturing plants from 1979±92. We identi®ed 46 separate local air regulations, many affecting multiple industries, and tracked their adoption and compliance dates as well as dates of increased stringency. We used local regulatory code books, the SCAQMD library, interviews with regulators and regulatees to establish the timing and coverage of regulations. Regulations were matched to industries using the text of the regulation, our understanding of production technologies, and information provided by South Coast regulators.[14]

Manufacturing plants located in Texas and Louisiana are our primary comparison group because the composition of industry in those states is similar to that

[13] A plant is a physical location engaged in a speci®c line of business. Plants with 20 or more employees are required to submit a survey form to the Census, while smaller plants are often enumerated using payroll and sales information from the Social Security Administration and the Internal Revenue Service. Imputed plants account for approximately 2.2% of value added (United States Bureau of the Census, 1993).

[14] Industries covered are in SIC codes 2051-53, 2426, 2431, 2451-52, 2819, 2820-24, 2834, 2843-44, 2851, 2873, 2911, 2952, 2999, 3221, 3229, 3231, 3241, 3271-73, 3315, 3357, 3411, 3452, 3652, 3674, 3711, 3713-16, 3721, 3724, 3728, 3731-32, 3761, 3764, 3769.

in the South Coast, but air pollution regulations are less stringent.[15] For alternate comparison regions we used ozone attainment/non-attainment regions according to their 1987 status.[16,17]

Coding regulations carries with it an inherent danger of bias. Regulations have enough technical attributes that coding involves numerous subjective judgements. For instance, a regulation requiring capital investment with compliance early in the year will force a plant to invest during the previous year, so it is coded as occurring in the previous year. If the researcher carrying out the coding has even a vague idea of the pattern to be explained, then subjective judgement implies a danger of (inadvertently) `data mining', by coding the data in a way that will help explain variation in the left hand side variable (in our case, employment). Our solution for is to `sequester' the data, not allowing the researcher who codes the regulations to observe the left hand side variables. We believe that this method of sequestering the data is *crucial* to obtain unbiased inferences from micro-regulatory data, especially in a case like ours in which the collection and coding of regulations is an expensive activity which does not lend itself to corroboration by replication.

We developed an exhaustive coding of signi®cant South Coast regulations for the 1979±92 period. To achieve precision we interviewed regulators and a sample of regulatees both personally and by telephone. Regulations are concentrated in heavy industry (paper, chemicals, petrochemicals, glass, cement, and transport). Regulatory data were matched to each of the two panels of plants (ASM-PACE and COM). For each plant-year we measure the number of new regulations adopted, new regulations that must be complied with and the number of

[15]In discussions with several individuals, we found that this opinion was widely held by regulators in the South Coast as well as plant engineers in companies with plants in both the South Coast and either Texas or Louisiana.When a direct comparison was possible between regulations in the South Coast and those in Texas and Louisiana, South Coast regulations were clearly more stringent B between two and ten times more stringent on a per unit emissions standard basis. For example the SCAQMD ([1159) requires that NO_2 emissions from nitric acid units be no more than 3 pounds per ton of acid per 60 min whereas in Louisiana the limit is 6.5 pounds per ton. At present, there are no other speci®c regulations for nitrous oxide emissions in Louisiana other than those for nitric acid units. Gas ®red steam generators in the South Coast ([1146, 1146.1) are limited to between 30 and 40 ppm per MMBTUs of heat input (0.037±0.04 lbs per MMBTUs of heat input) but in Texas (in the Dallas/Fort Worth ACQR and Houston/Galveston ACQR) the limit is 0.25±0.7 lbs/MMBTUs. The cost of the South Coast regulation on gas ®red steam generators is estimated at between $9161 and $16 635 per ton in 1990$, or $3.9±$4.6M per year.

[16]Another measure of regulatory stringency is effort expended by the regulators, including enforcement activities, as proxied by budgets. The SCAQMD's budget is, on average, eight times as large as that of the Louisiana Air Quality Program and in 1999 was approximately the same size as that of all of Texas for their Clean Air Account. Thus the South Coast spends approximately 2.5 times as much per capita on air pollution control as Louisiana and 1.3 times as much as Texas.

[17]These data were kindly provided by Randy Becker of the Census Bureau Center for Economic Studies.

regulations with increases in stringency. For example, Rule 1112 applies an emission standard to NO_2 emissions from cement kilns. The Rule was adopted in 1982 and had a date of compliance in 1986. These regulatory data are available from the authors upon request.

For comparison plants we include in each panel all US manufacturing plants located outside of the Basin in industries that would have been affected by SCAQMD regulations had they been located there.

5. Estimation

5.1. *Econometrics*

We are interested in estimating the effect of the South Coast regulations on employment in regulated plants. We first describe the estimating equation and then discuss how we deal with potential sources of bias.

The effect of regulation on labor demand, given by Eq. (3), can be taken to data as:

$$L_{ijrt} = \delta_i + \phi_t + \mu R_{jrt} + \eta_{jt} + \nu_{rt} + \varepsilon_{ijrt}. \qquad (3')$$

The unit of observation is a plant-year. The parameter μ is the effect of regulation on employment; δ_i is a plant-specific employment effect for $i = 1,\dots,N_t$ plants; ϕ_t is a year effect for years $t = 1,\dots,T$; η_{jt} is an industry effect for industries $j = 1,\dots,J$; and ν_{rt} is a region effect for regions $r = 1..R$. We eliminate the plant-specific effect by differencing to yield:

$$\Delta L_{ijrt} = \Delta\phi_t + \mu\Delta R_{jrt} + \Delta\eta_j + \Delta\nu_r + \Delta\varepsilon_{ijrt} \qquad (5)$$

assuming employment trends $\Delta\eta$ and $\Delta\nu$ in industries and regions respectively. The parameter of interest, μ, can be consistently estimated if $Cov(\Delta R_{jrt}, \Delta\varepsilon_{ijrt}) = 0$.

The assumed orthogonality of regulatory change with unexplained variation in employment change is conditional on year, industry and region indicators. This conditioning is critical. Regulatory change is certainly bunched in particular years, which typically have their own secular employment change. Particular industries and regions also have their own secular patterns of employment change. The orthogonality assumption is a claim that regulatory changes are correlated with employment changes only through the causal effect μ, once the common effects of time, industry and region are taken into account.

The effects of *local* regulatory change on employment are described by μ. They provide a tool for local policymakers by predicting the local employment effects of similar regulatory changes (e.g. tightening standards for airborne pollutants). The effect of a regulation can be interpreted as the marginal effect of imposing the (more stringent) SCAQMD regulations over and above the average level of regulation (Federal and State) these industries face in the rest of the country. Since the level of regulation varies from region to region, the estimated effects should be interpreted as an average of separate cross-region comparisons.

Before turning to results, we discuss three potential sources of bias we believe apply to this literature and explain how our identi®cation strategy deals with them.

5.1.1. *Selection bias*

This is the ®rst effort we know of to estimate the effects of local air quality regulations directly in an analysis including comparison plants. An alternative approach which indirectly measures these effects is to estimate (2) directly, using abatement activity (Z) as a covariate in a labor demand function. That approach avoids the considerable effort described above of quantifying regulations but is susceptible to selection bias. Plants may carry out PACE voluntarily even in the absence of regulation, a phenomenon that is probably more common at plants that anticipate small disruptions due to PACE (Gray, 1987). Such a selection bias would tend to yield estimates which understate negative employment effects of PACE *forced* by regulation, which is the relevant parameter for policy analysis. Selection bias may explain the surprising Gray and Shadbegian (1993b) result that PACE is positively correlated with employment.

5.1.2. *Measurement error*

PACE is dif®cult to measure for two reasons. First, the distinction between investments in pollution abatement capital and other new capital is often subtle (Jaffe et al., 1995). For example, new equipment is frequently both more ef®cient and cleaner. Second, the survey form de®nes PACE as the difference between capital investment and the counterfactual capital investment that would be made in the absence of the need to abate. While this is exactly the de®nition an analyst would like, it is a dif®cult question for a respondent to answer. After years of air quality regulation that counterfactual may be dif®cult to imagine, as it is far removed from experience. This is a type of measurement error, which, in the regression of employment on abatement, will generally bias coef®cient estimates towards zero.

5.1.3. *Anticipatory response*

An additional problem in estimating the effects of any regulatory change is that measurement of treatment effects may be frustrated by changes in behavior in anticipation of regulatory change (Meyer, 1995). For that purpose we measure not only compliance dates but also the date in which a regulation is introduced into law, typically a few years earlier. If plants adjust behavior in anticipation of required compliance with a regulation we would expect to see that adjustment in the adoption year. We include an indicator for that date in the set of regressors to measure anticipatory reaction to regulation. We also questioned engineers and managers, who indicated that anticipatory abatement investment is unlikely, as compliance typically involves high costs which they would not incur until it was absolutely necessary. We estimate (5) in both annual and 5-year differences to capture both short term and long term responses.

We describe regulations using three binary indicators, one for the year of required compliance, a second for the year in which an existing regulation became more stringent and a third for the date of adoption of the regulation.[18] For each indicator the coef®cient estimates the average treatment effect, averaged over all South Coast regulations introduced during this period.

5.2. *Result from a balanced panel*

Our ASM-PACE panel contains 18 540 plant-years in industries that would be subject to South Coast regulations if they were located in the LA air basin. They represent 60 500 plants in the population. The panel contains data for 1979±1991, excluding 1983 and 1987 (for which data were destroyed and not collected, respectively). Table 2 reports means and standard deviations, weighted by sampling weights to re¯ect population statistics. The means indicate that in these industries abatement investment and operating costs are high, averaging $103 000 and $271 000 per plant, respectively. Abatement costs vary considerably among plants, with standard deviations an order of magnitude larger than the means. This re¯ects the large costs incurred by a small number of petrochemical and chemical plants. Note that 5.3% of plant-years are located in the LA Basin. The compliance indicator averages 1.36%. The average year to year change in employment is 2 10, which re¯ects the national contraction in manufacturing employment in heavy industry over the 1980s. In comparison with plants in the same industries in other regions, South Coast plants are smaller and have higher proportions of abatement investment and operating cost to value added.[19]

We begin by presenting the estimated effects of regulation on employment from Eq. (5), using changes in regulation to explain year to year changes in employment. Regulatory changes, $\mathrm{D}R_{jrt}$, take values of zero, one and sometimes two for South Coast plants and are always zero for plants in other regions. The vector $\mathrm{D}R_{jrt}$ includes new regulations adopted (but which require no immediate action), new compliance dates and dates of increased stringency of existing regulations. While we expect the effects of regulation to occur in compliance years and years of increased stringency, the adoption year indicator is included to allow for

[18] Strictly speaking these are counts, since more than one new regulation sometimes applies to a plant in a given year, so that $\mathrm{D}R$, while generally binary, can take values of up to 4 over the 5-year differences reported for the CM below. Coef®cients should be interpreted as the average effect of a single regulation.

[19] High abatement investments and operating costs in the South Coast are mostly due to increases among re®neries beginning in the mid 1980s, as illustrated in Fig. 1.

Table 2
PACE descriptive statistics[a]

Variable	All counties	Los Angeles Basin	Ozone attainment counties	Ozone nonattainment counties	Texas± Louisiana
Air pollution abatement					
Investment					
Net[b]	103	249	56	134	213
	(1875)	(2961)	(1041)	(2330)	(2745)
Gross[b]	141	303	80	184	316
Process[b]	43	134	21	55	116
End of Line[b]	99	168	59	129	200
Costs[b]	271	539	132	379	896
	(2760)	(3721)	(1039)	(3677)	(6664)
Change[b]	0.4	14	2 4.3	1.8	2 10
	(1398)	(1340)	(866)	(1796)	(3279)
Regulatory change (counts)					
Adoption	0.0073	0.143	±	±	±
Compliance	0.0136	0.269	±	±	±
Increased stringency	0.0027	0.053	±	±	±
Value added[b]	25 666	16 398	18 196	33 281	29 290
	(100 501)	(53 005)	(68 987)	(124 925)	(95 731)
Change[b]	2 598	2 1195	2 368	2 741	2 13 334
	(50 541)	(42 123)	(36 580)	(62 338)	(71 423)
Employment	267	178	205	323	212
	(867)	(350)	(585)	(998)	(533)
Change	2 10	2 6	2 5	2 15	2 6
	(173)	(117)	(98)	(221)	(106)
LA Air Basin (%)	5.3	100	±	±	±
Observations	18 540	964	6973	9483	2086

[a] Notes: Means weighted by LRD-PACE sampling weights. In all, 18 540 sample observations represent 60 500 plant-years in the population of manufacturing plants over the sample period 1979±1991, excepting 1983 and 1987 when data is unavailable. Standard deviations in parentheses. Attainment indicates that the county is below the federal ozone guideline for ambient air quality in 1987. All plants in the LA air basin are in nonattainment counties. Attainment/nonattainment classi®cation is not available for a small number of counties.

[b] Thousands of 1991 dollars de¯ated by PPI.

possible anticipatory response. Note that regulations vary widely in their speci®cations and potential effects so that estimated effects should be interpreted as average treatment effects.

Table 3 reports estimated coef®cients. Employment effects are very small, generally positive, but not statistically different from zero.[20] The ®rst three

[20] We chose to estimate in levels so that aggregation to estimate program effects would be straight-forward. These results and those that follow are qualitatively unchanged when estimated using differences in logarithms. We did not experiment with other speci®cations when exit and entry were involved.

Table 3
The effect of regulation on employment[a]

	All counties			
Adoption	0.8	2 5.2	4.6	2 6.1
	(7.1)	(7.0)	(6.9)	(8.6)
(3 Oil)[b]	±	±	±	6.4
				(13.5)
Compliance	1.3	3.7	3.8	4.5
	(3.4)	(4.3)	(4.1)	(5.0)
(3 Oil)	±	±	±	2 2.8
				(6.8)
Increased stringency	2 8.5	5.6	4.6	2.9
	(6.7)	(12.3)	(12.0)	(15.8)
(x Oil)	±	±	±	7.2
				(17.1)
36 industry indicators	±	1	1	1
50 state indicators	±	±	1	1
N	18 540	18 540	18 540	18 540
R^2	0.011	0.023	0.026	0.026
Program effect[c]	2 292	3948	3862	4100
	(2938)	(3590)	(3487)	(3413)

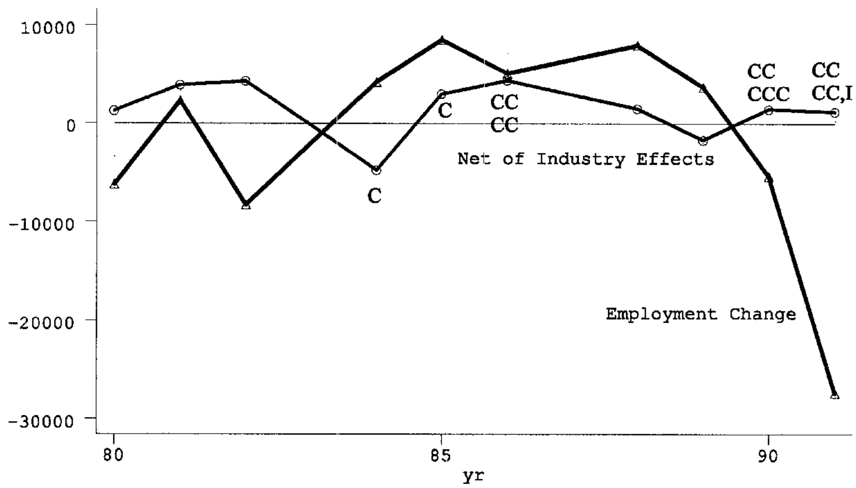
[a] The dependent variable is plant-level employment, ®rst-differenced. Weighted by PACE-LRD sampling weights. Each estimate includes 9-year indicators and an indicator for the South Coast Air Quality Management District. Standard errors in parentheses are heteroskedasticity consistent. The mean employment change is 2 10.

[b] ` 3 Oil' is in each instance a variable set to one if a regulatory change (e.g. adoption) occurred *and* it affected the petroleum industry (SIC code 2911).

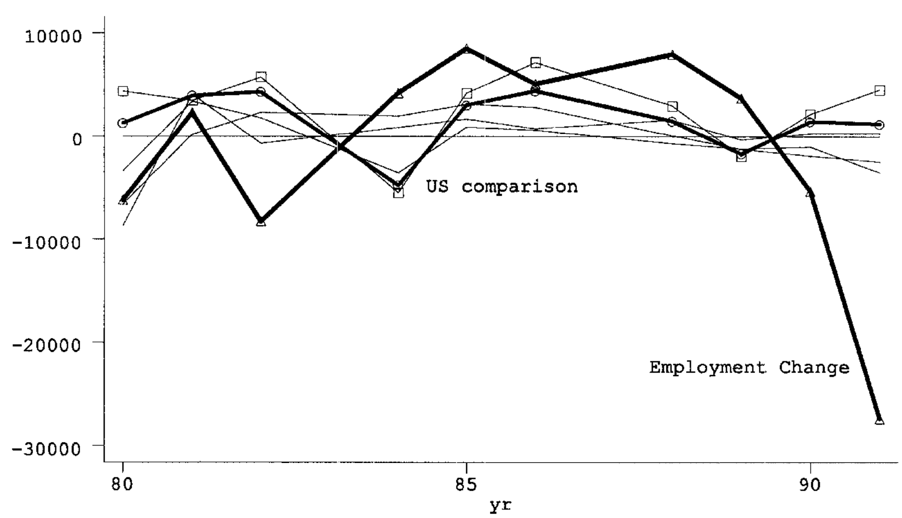
[c] Program effects are the sum of affected plants multiplied by estimated coef®cients for compliance, increased stringency and their interactions with oil, where applicable.

columns indicate that these small estimated coef®cients are robust to controlling for industry and state effects. The speci®cation allowing industry and state speci®c year to year employment changes yields point estimates of an additional 3.8 employees in compliance years and an increase of 4.6 employees in years of increased stringency (which occur about one ®fth as often). These estimates do not rule out zero or negative effects of regulation on employment, but they do rule out the large negative effects (`job loss') often attributed to environmental regulation in the popular press. There is no evidence that adoption dates matter, a point to which we return, below. As so much abatement cost is incurred by re®neries, we report separate effects for re®neries and non-re®neries, which are also all small.

Panel A of Fig. 3 illustrates why these estimates are controversial and how the use of comparison plants in¯uences inference. It plots employment change in the 36 regulated industries in the South Coast from 1980 through 1991. The accelerated decline in employment in 1990 and 1991 of tens of thousands of workers has been attributed to air quality regulations. Each new compliance date with a South Coast regulation is marked with a `C' and the single increased



A. Employment Change Net of Industry Effects



B.Employment Change - Alternative Comparisons

Fig. 3. Employment change in the South Coast Air Basin. Note: The ®gure illustrates the effect of allowing industry-speci®c trends in employment to explain employment change in the 36 regulated industries of the South Coast. Panel A plots aggregate employment change and the sum of residuals for South Coast plants from a regression of changes in employment on industry and year effects (as in Eq. (5) but excluding regulation variables. The sample includes the 36 industries in all of US manufacturing, as in Table 3. `C' and `I' indicate compliance and increased stringency dates for nonre®neries. Panel B plots aggregate employment and sums of South Coast residuals from a regression of changes in employment on industry and year effects for ®ve different samples (as in Table 4) the US, the South Coast, Ozone Attainment regions, Ozone Nonattainment regions and Texas±Louisiana. Aggregates are calculated using PACE-LRD sampling weights.

stringency date with an `I'. Only regulations applying outside re®neries (SIC 29) are marked as re®nery employment is relatively small and shows little variation over this period. Ten of 16 compliance and increased stringency dates occur in 1990±91, so it is not surprising that environmental regulations were ®ngered as the cause of the employment decline.

The second series illuminates how the estimates in Table 3 exonerate air quality regulations. We constructed that series by ®rst regressing changes in employment on year and industry indicators, as in column 2 of Table 3, excluding regulation variables.We then summed the employment change residuals for the South Coast, creating an employment change series net of national period effects and national industry-speci®c employment trends. That series shows no decrease in employment in 1990 or 1991, indicating that the heavy job loss experienced in the South Coast in those years is due to a high proportion of declining industries. Once national industry-employment trends are netted out, there is little job loss left for local regulations to explain.

The coef®cients on compliance and increased stringency dates can be used to estimate the cumulative effect of the set of 1980±1991 environmental regulations of manufacturing plants in the South Coast, reported in the last row as the `program effect.' The point estimate using the speci®cation in the rightmost column is a 4100 person increase in employment with a 95% con®dence interval ranging from 3570 jobs lost to 11 770 jobs gained. Using the lower bound of that con®dence interval as a worst case, job loss due to regulation was probably less than 3570 Ð a small number, having the same order of magnitude as the estimated *annual* rate of excess deaths from being out of compliance with national standards in the mid 1980s.

Interpretation of these coef®cients as the causal effects of regulation depends critically on the assumption that, in the absence of regulation, the treated plants would have behaved like the comparison plants, conditional on industry, region and year. Industry indicators are good predictors of the propensity of comparison plants to be treated (i.e. subject to South Coast regulations) had they been located in the South Coast, since industries share production technologies across regions [21] and since the incidence of regulations is based either on process or directly on industry.

A possible weakness of our approach is that a small number of our comparison plants may be subject to some degree of region-speci®c environmental regulation that is promulgated at the state level.[22] Thus, the treatment effects estimated must be interpreted as the effects of the difference between South Coast regulations and the average level of a small number of location-speci®c state regulations in comparison regions. We address this issue in two ways. First, since location-

[21] In interviews, production engineers indicated that they used largely the same capital goods as competing ®rms and as plants in the same ®rm in other regions.

[22] When these location-speci®c regulations exist, they are targeted at plants in non-compliance areas.

speci®c state regulations are triggered by non-compliance with federal air quality standards, we compare (the treated) South Coast plants with plants in both attainment and non-attainment regions for federal ozone standards in 1987. Since we expect that non-attainment regions have more stringent local regulations, on average, the contrast between their outcomes and those of the South Coast plants is particularly interesting. These are also the regions for which the results are most relevant, as they are most likely to adopt the more stringent South Coast regulations.

Our second approach is to draw comparison plants from Texas and Louisiana, which have a pollution intensive industrial mix, with large petroleum re®ning and heavy industry sectors. Unlike the South Coast, these two states bene®t from topological and climactic conditions that make them much less prone to accumulate ground level ozone.We directly compared state regulations in these two states with the local regulations in the South Coast and found that they were much less stringent (see footnotes 15 and 16).

Table 4 describes the outcome of both approaches. The leftmost column reports estimated coef®cients using the South Coast plants only, with comparison plant-years limited to the same plants in years for which they do not have compliance and increased stringency dates. Point estimates suggest that regulated plants had faster employment growth in years with new compliance and increased stringency

Table 4
The effect of regulation on employment: alternate comparison regions[a]

	LA Air Basin and . . .				
	LA Air Basin only	Ozone attainment counties	Ozone nonattainment counties	Texas±Louisiana	All counties
Adoption	21.9	23.9	24.0	0.5	4.6
	(9.6)	(6.8)	(7.4)	(7.0)	(6.9)
Compliance	5.8	3.9	3.9	3.4	3.8
	(4.6)	(3.6)	(4.9)	(4.1)	(4.1)
Increased stringency	9.0	23.5	13.8	21.2	4.6
	(16.4)	(8.6)	(15.2)	(12.5)	(12.0)
36 industry indicators	1	1	1	1	1
50 state indicators	±	1	1	±	1
N	964	7937	10 447	3050	18 540
R^2	0.050	0.023	0.033	0.022	0.026
Program effect[b]	6219	2643	5424	2652	3862
	(3870)	(3188)	(4094)	(3673)	(3487)

[a] Dependent variable is plant-level employment, ®rst-differenced.Weighted by PACE-LRD sampling weights. Each estimate includes 9-year indicators and an indicator for the South Coast Air Quality Management District. Standard errors in parentheses are heteroskedasticity consistent. The mean employment change is 210.

[b] Program effects are the sum of affected plants multiplied by estimated coef®cients for compliance, increased stringency and their interactions with oil, where applicable.

dates than in other years, (thought the effect is not statistically signi®cant). The other columns report estimates which contrast employment growth for South Coast plants with employment change in plants in the same industries in comparison regions. That contrast generally reduces the estimated employment effects slightly, but does not make them signi®cantly negative in any case. Estimated program effects are all positive, with lower bounds on their con®dence intervals predicting small employment losses at worst. The conclusion from comparisons with plants in attainment counties, non-attainment counties and the relatively unregulated States of Texas and Louisiana is always the same: *employment effects are fairly precisely estimated and small*. This robustness to the choice of comparison groups is illustrated in panel B of Fig. 3, which plots employment change as in panel A and net employment change using each of the ®ve comparison groups in Table 4. All ®ve comparisons yield the same conclusion: secular industry effects alone can explain the rapid decline in South Coast employment in 1990 and 1991. This is true even when these trends are estimated using as a comparison region ozone attainment counties that were subject to neither South Coast nor other local regulations.

Considering these small and statistically insigni®cant employment effects a legitimate question is whether environmental regulation did anything economically signi®cant in manufacturing plants. In terms of Eq. (4), was there a `®rst stage' effect of regulation on abatement and output? Fig. 2 provided one response, showing that regulations induced reduced emissions. It reported sizeable NO_x emissions reductions in regulated South Coast plants after 1985, in both re®neries and non-re®neries, much faster than the reductions in unregulated South Coast plants. Table 5 provides another response, showing the result of estimating the analogous equation to (5) for abatement investment. It is estimated in ®rst differences with year to year changes in abatement capital (net abatement investment) on the left hand side and DR on the right. The results show that compliance and increases in regulatory stringency have large and signi®cant effects on abatement investment. The units are thousands of dollars (constant 1991$) so that the coef®cient on compliance in the leftmost column implies $583 000 of capital investment induced by each new regulation per affected plant. The estimated effect of increased stringency is larger, but somewhat less precisely estimated. The point estimates in column 1 indicates that increased stringency induces an additional $2M in investment per plant. These results are robust to changes in comparison regions. Those coef®cients clearly indicate that the South Coast regulations imposed large costs on manufacturing plants.

The ®rst row indicates no evidence that adoption of regulations has any effect on abatement investment. The evidence is weak, but consistent with the opinion of environmental engineers we interviewed, who reported that anticipatory investment was unlikely because the high cost of abatement investment.

Our key results on the effect of regulation on employment in Tables 3 and 4 above can be thought of as reduced form estimates of Eq. (4), for which the

Table 5
The effect of regulation on air pollution abatement investment[a]

	Ozone attainment counties	Ozone nonattainment counties	Texas± Louisiana	All counties	All counties
Adoption	2 169	2 221	2 299	2 170	2 37
	(194)	(178)	(193)	(187)	(43)
(3 Oil)[b]	±	±	±	±	442
					(572)
Compliance	583	522	566	553	2 17
	(238)	(225)	(234)	(234)	(39)
(3 Oil)	±	±	±	±	2730
					(1052)
Increased stringency	1988	1676	1882	1807	2 256
	(1056)	(1026)	(1063)	(1044)	(147)
(3 Oil)	±	±	±	±	7016
					(2937)
36 industry indicators	1	1	1	1	1
50 state indicators	1	1	±	1	1
N	7937	10 447	3050	18 540	18 540
R^2	0.057	0.057	0.073	0.041	0.058
F-statistic[c]	4.61	3.96	4.10	4.24	4.77
(a)	(0.01)	(0.02)	(0.02)	(0.01)	(0.001)
Program effect[d]	802 907	701 842	771 881	748 772	415 164
	(265 037)	(251 221)	(271 544)	(258 065)	(120 721)

[a] Dependent variable is plant-level pollution abatement capital investment (air), ®rst-differenced. Weighted by PACE-LRD sampling weights. Each estimate includes 9-year indicators and an indicator for the South Coast district. Standard errors in parentheses are heteroskedasticity consistent. The mean of net air pollution abatement investment is 103 (1000s of 1991$s).

[b] ` 3 Oil' is in each instance a variable set to one if a regulatory change (e.g. adoption) occurred *and* it affected the petroleum industry (SIC code 2911).

[c] The F-statistic reports the results of an F-test of the hypothesis that the coef®cients on compliance and increased stringency are jointly equal to zero. The number in parentheses is the signi®cance level at which that hypothesis can be rejected.

[d] Program effects are the sum of affected plants multiplied by estimated coef®cients for compliance, increased stringency and their interactions with oil, where applicable.

estimates in Table 5 are a ®rst stage. Those reduced form estimates would be subject to the same biases we seek to avoid in OLS estimates if the ®rst stage had only a weak correlation between regulatory change and investment (Bound et al., 1995). For that reason we report near the bottom of Table 5 an F-test of the joint hypothesis that the coef®cients on both compliance and increased stringency are zero. The F-statistics are all around four, indicating negligible bias in the reduced form (Bound et al., 1995, Table A1).

The rightmost column reports estimates allowing separate slopes for oil re®neries, implying that the positive aggregate effects of investment are entirely due to multimillion dollar investments by oil re®neries (SIC 2911), with the effects for other industries insigni®cantly different from zero. These contrast with the results in Fig. 2, which ®nd NO_x reductions in both re®neries and in other regulated industries.

Table 6 repeats that procedure for abatement operating costs and value added respectively. We ®nd no evidence that regulatory change has any effect on abatement operating costs or value added. The data may be uninformative because differencing the levels of abatement cost and value added exacerbates measurement error. Measurement of abatement operating costs is especially suspect because its variation from year to year seems to be unreasonably high.

Taken together, the results in Tables 3±5, and Fig. 2 provide an interesting contrast. Though air quality regulation induced large investments in abatement capital in oil re®neries, and NO_x reductions in general, what little effect it had on employment seems positive, if anything. The evidence of a `®rst stage' effect of regulations on abatement, together with evidence of reduced emissions, indicates that the regulations did indeed impose real costs on manufacturing ®rms, but did so with no detectable loss of employment.

Table 6
The effect of regulation on abatement operating costs and value added[a]

	Air pollution abatement operating costs ($1000s)			Value added ($1000s)		
	LA, Texas, Louisiana	All counties	All counties	LA, Texas, Louisiana	All counties	All counties
Adoption	2 55	2 85	2 13	2 13 315	2 11 958	2 533
	(285)	(259)	(14)	(5567)	(5178)	(1639)
(3 Oil)[b]	±	±	2 158	±	±	2 46 123
			(1101)			(19 811)
Compliance	71	86	2 12	6320	5748	486
	(126)	(113)	(10)	(3419)	(3210)	(963)
(3 Oil)	±	±	688	±	±	16 737
			(451)			(16 027)
Increased stringency	2 500	2 508	2 15	2 14	2 992	2 1955
	(452)	(416)	(19)	(10 238)	(8983)	(1663)
(3 Oil)	±	±	2 2168	±	±	1094
			(1454)			(35 642)
36 industry indicators	1	1	1	1	1	1
50 state indicators	±	1	1	±	1	1
N	3050	18 540	18 540	3050	18 540	18 540
R^2	0.003	0.001	0.002	0.012	0.010	0.011

[a] Dependent variable is plant-level pollution abatement operating and maintenance costs (air), ®rst-differenced. Weighted by PACE-LRD sampling weights. Each estimate includes 9-year indicators and an indicator for the South Coast Air Quality Management District. Standard errors in parentheses are heteroskedasticity consistent. The mean change in air pollution operating costs is 0.4 (1000s of 1991$s) and the mean change in value added is 598 (1000s of 1991$s).

[b] ` 3 Oil' is in each instance a variable set to one if a regulatory change (e.g. adoption) occurred *and* it affected the petroleum industry (SIC code 2911).

5.3. *Entry and exit analysis using the census of manufactures*

Environmental regulation may in¯uence employment by inducing plants to exit or dissuading them from entering into production. A limitation of the ASM results above is that entry and exit are not recorded in a panel of continuing plants so that potential employment effects of regulation have gone unmeasured.[23] Cost-minimizing behavior predicts that employment effects are more likely to be negative through induced exit and dissuaded entry than they are for continuing plants (Eq. (4)), since technical complementarity between abatement and employment requires production.[24] To capture the effects of regulation through exit and dissuaded entry we turn to the quintennial Census of Manufactures, the most complete data on manufacturing employment available from any source. As before, our sub-population includes plants which would have been subject to South Coast regulations had they been located in the South Coast. Comparison regions represent counterfactual patterns of employment change, including entry and exit, which would have occurred in the South Coast in the absence of regulations. Pooling all three types of employment change, we estimate the effects of regulation through forced exit, dissuaded entry and changes in employment in continuing plants.

One weakness of the Census to Census comparison is that over a 5-year period other events may occur in regulated industries in the LA Basin or elsewhere that confound analysis of the effects of regulation. One such event is the sharp decrease in orders for defense-related goods as the federal government reduced spending on `Star Wars' and other programs. This led to considerable job loss in the aerospace industry, which is disproportionately concentrated in Southern California, an industry that was subject to two relatively minor environmental regulations in the 1987±92 period. Most of these industries were affected by one VOC regulation concerning coatings, which had a compliance date of January 1993, long after their sharp downturn in employment. To control for ¯uctuations in defense procurement we use a sub-population of regulated industries in the CM to exclude the aerospace and shipbuilding industries.[25]

The effect of changes in regulation on changes in employment in Eq. (5) is estimated for departing and entering plants as follows: plants entering are assigned zero employment in the census year before they appear and plants departing are assigned zero employment in the census year after they exit. Employment levels are then used to calculate ®ve year differences for all plants, including continuing

[23] The Annual Survey of Manufacturers changes its sample of smaller plants periodically so that entry and exit are not well observed and are practically indistinguishable from plants joining and leaving the sample.

[24] Regulation could also induce entry of plants which produce abatement producing equipment. None of the industries covered by the South Coast regulations fall into that category.

[25] Berman and Bui (1997) provide further analysis of employment trends in defense industries.

Table 7
Census of manufactures: regulated industries 1977, 1982, 1987, 1992 excluding aerospace and shipbuilding[a]

Variable	All counties	Los Angeles Basin	Ozone attainment counties	Ozone nonattainment counties	Texas± Louisiana
Regulatory change					
Adoption	0.023	0.34	±	±	±
Compliance	0.037	0.54	±	±	±
Increased stringency	0.008	0.12	±	±	±
Value added[b]	5641	3693	4866	6846	7252
	(43 812)	(17 584)	(53 685)	(124 925)	(40 402)
Change[b]	1572	896	1513	1811	1920
	(24 204)	(10 733)	(28 584)	(62 338)	(24 189)
Employment	68.6	51.5	65.1	76.6	69.5
	(328.2)	(148)	(397.6)	(998)	(245.5)
Change	2 2.1	2 1.5	1.9	2 5.3	2 0.8
	(158.6)	(81.7)	(195.9)	(221)	(113.4)
LA Air Basin (%)	6.7	100	±	±	±
Observations	142 613	9604	68 294	77 898	10 933

[a] A total of 142 613 observations of 5-year differences, covering the periods 1977±82, 82±87, 87±92. Value added and employment levels are based on observations for the years 1982, 1987 and 1992. The sub-population includes all 46 regulated industries listed in Table A2 with the exception of six aerospace industries and shipbuilding (SIC codes 3721, 3724, 3728, 3761, 3764, 3769 and 3731).
[b] Value added is reported in thousands of constant 1991 dollars.

plants. Note that this method also allows an estimate of a longer term response over the 5-year intervals.

Table 7 reports three periods of 5-year changes in employment: 1977±82, 1982±87 and 1987±92. Average employment change for a plant over these 5-year periods was 2 2.1 employees, including employment increases for entrants and decreases for exits. Regulatory change is added up for the 5-year intervals between Census years. Plants outside the South Coast are assigned no increase in regulations over the 5-year intervals. Plants in the South Coast had between zero and ®ve new compliance dates for regulations. The average for all plants was 0.037 new compliance dates and 0.008 dates of increased stringency.

As in the PACE-LRD sample, comparison regions are chosen with varying levels of state regulation. Table 8 reports estimates of Eq. (5) which allow for exit and entry. The ®rst column reports results including all (non-defense) plants, including entrants and exiting plants. Employment effects per new compliance regulation vary from 1 2.8 (for the Texas±Louisiana comparison group) to 2 2.4 (for non-attainment counties). Effects of increased stringency vary from 1 5.6 to 2 3.2.

The fourth column reports results for the entering and exiting plants only, using

Table 8
Effects of regulation on employment between census years 1977±82, 1982±87, 1987±92: alternative comparison regions[a]

	LA Air Basin and ...					
	Ozone attainment counties	Ozone nonattainment counties	Texas± Louisiana	Texas± Louisiana entry/exit only	All counties	All counties (including aerospace)
Adoption	2 0.5	5.4	2 2.9	2 1.8	3.4	6.6
	(2.1)	(2.2)	(2.2)	(2.8)	(2.0)	(6.1)
Compliance	2 1.1	2 2.4	2.8	3.1	2 2.0	2 8.3
	(1.3)	(1.4)	(1.4)	(1.9)	(1.3)	(4.7)
Increased stringency	2 3.2	5.6	2 2.2	2 3.3	1.6	11.3
	(3.7)	(3.8)	(4.2)	(6.0)	(3.5)	(6.1)
Industry indicators[b]	1	1	1	1	1	1
State indicators[c]	1	1	1	1	1	1
N	63 154	77 898	20 537	12 107	142 613	151 908
R^2	0.009	0.008	0.009	0.015	0.005	0.003
Program effect[d]	2 9589	2 6140	12 266	7047	2 8860	2 34 286
	(8545)	(8678)	(9632)	(8348)	(8303)	(23 479)

[a] Heteroskedasticity-consistent standard errors in parentheses. The sample is described in Table 7. Dependent variable is 5-year changes in plant-level employment.

[b] All columns include 39 four-digit industry indicators except for the rightmost, which includes an additional seven in aerospace and shipbuilding. These seven industries are subject to a relatively minor VOC regulation with a compliance date in the ®rst quarter of 1993.

[c] From the leftmost column, the number of state indicators is respectively 42, 32, 2, 51 and 51 (including Puerto Rico). All columns include a separate indicator of the South Coast.

[d] Program effects are the sum of affected plants multiplied by estimated coef®cients for compliance and increased stringency.

the Louisiana±Texas comparison group for which we are most con®dent that there is relatively little local air quality regulation. Surprisingly, we ®nd coef®cients of similar size for exitors and entrants on the one hand and for continuing plants on the other.[26] The effects for both entry/exit and continuing plants are small, positive and not statistically distinct from zero. This positive point estimate on compliance is a little surprising for the exit/entry sample (though statistically insigni®cant) especially since it is larger than that estimated for continuing plants. (This is true

[26] There is a large potential for misclassi®cation of continuing plants as entrants and exits in the Census but that misclassi®cation should not bias our estimates. Though the Census includes all plants it is not designed for longitudinal study, so that plant identi®ers may change between waves of the Census, leading a continuing plant to be falsely classi®ed as an exitor and an entrant. For example, if a continuing plant has employment decrease from 55 to 50 employees over the 5 years between Censuses employment change should be recorded as 2 5. If its identi®cation number is changed between Census years it will be mis-classi®ed as an exiting plant with 55 employees and an entrant with 50. We can't think of a reason why this misclassi®cation would be correlated with regulatory change so we are con®dent that it does not bias the reported estimates.

for the full sample as well, though not reported.) While it may be due to misclassi®cation of continuing plants as exit/entry combinations, it underlines the ®nding of no large negative employment effect through induced entry and exit. Other comparisons (not reported) yield similar results.

The ®nal column reports the effect of ignoring the bias due to a procurement drop in the South Coast and including defense-related industries. That estimate would imply large negative employment effects. As discussed in connection with Fig. 3, the confusion between the effects of decreased defense contracts and environmental regulation may be why regulation was falsely implicated in the employment loss in South Coast manufacturing.

Overall, these coef®cient estimates are more negative but statistically indistinguishable from the estimates based on annual employment and regulatory change reported in Tables 3 and 4 above, providing corroboration of those results in a different data set, over a longer time period and including exit and entry effects. The similarity of the annual and quintennial results is evidence that these estimated employment effects are not subject to measurement error bias or confounded by lagged or anticipated response. As in the annual data, neither of these ®gures is statistically different from zero, but the standard error is small enough to rule out large employment effects, both positive and negative.

The coef®cients allow a fairly precise estimate of the cumulative effect on employment of the 1980±92 period of air quality regulation in the South Coast: point estimates range from 9600 jobs lost when compared with attainment counties, to 12 300 jobs created when compared to Louisiana and Texas (excluding the last column which includes aerospace industries). The 95% con®dence interval in the worst case is [226 338, 7160] and in the best case is [26613, 31 145], so that at standard levels of con®dence we can bound the employment effects of the entire program between about 26 000 jobs lost and 31 000 jobs gained.[27]

Comparing the Census estimates of employment effects to those in the annual ASM-PACE samples, the latter are generally more positive. While the Census estimates have the advantage of broader coverage, including entry and exit effects, they are also reported at lower frequency, increasing the probability of a confounding secular industry±region±period event, such as the drop in defense procurements. Finally, they are not completely comparable since they include an extra year of regulation, in 1992. Nevertheless, the Census estimates generally reinforce the conclusion of small employment effects found in the ASM-PACE data in Tables 3 and 4. Air quality regulation in the South Coast did not cause large scale job loss even when dissuaded entry and exit and longer term adjustment are taken into account.

[27] Berman and Bui (1998) report several other speci®cations, all yielding small positive employment effects for the sample excluding aerospace.

6. Conclusion

The local air quality regulations introduced during 1979±92 in the Los Angeles Basin were not responsible for a large decline in employment. In fact, they probably increased labor demand slightly. We reach that conclusion by directly measuring regulations and comparing changes in employment in affected plants to those in comparison plants in the same industries but in regions not subject to South Coast regulations. Our ability to construct appropriate comparison groups in regions without local regulation is the key to identifying treatment effects and to establishing the robustness of these estimates.

Reduced form estimates alone are uninformative about why these employment effects are small. One possible explanation is that the program had no economic effect on the subject plants. That possibility, however, is ruled out by our considerable evidence of induced abatement investment in re®neries and of induced abatement of NO_x emissions in both regulated re®neries and regulated non-re®neries.

Another possible explanation for small employment effects is that South Coast regulations targeted capital intensive industries with relatively little employment. This is certainly true of oil re®neries but also true of chemicals, cement, transportation and other heavy manufacturing. Thus, our conclusions may extend to environmental regulation in other regions only to the extent that they affect capital-intensive industries (which they often do).

Plant visits and phone surveys support another explanation (suggested by theory) that, on the one hand, output effects of regulation may be small, while on the other, that labor and abatement activity are compliments. Most managers we spoke to thought that the introduction of abatement technology increased labor demand. While all complained about the nuisance of dealing with regulators and complying with regulations, few complained about lost demand for their product. We speculate that this is because these plants sell to local markets and face little competition from unregulated plants (in the oil and chemical industries).

Our estimates of zero employment effects contradict the conventional wisdom of employers (mostly outside of re®ning) that environmental regulation `costs jobs' (Goodstein, 1996) so a comment is in order. Beyond posturing in public debate, employers may honestly overestimate the job loss induced by a pervasive regulation by confusing the ®rm's product demand curve with that of the industry. The former is more price elastic due to competition from other ®rms. If all ®rms in the industry are faced with the same cost-increasing regulatory change and product demand is inelastic, the output of individual ®rms may be only slightly reduced. In that case, the negative effect on employment through the output elasticity of labor demand may well be dominated by a positive effect through the marginal rate of technical substitution between PACE and labor, leading to a net increases in employment as a result of regulation.

We also ®nd evidence that plants induced to respond to environmental regulation only do so at the latest possible moment Ð adoption dates have no discernible effect on a plant's investment whereas mandatory compliance dates have a strong effect. This is not surprising given the large capital investment associated with coming into compliance with a given regulation.

Though the public debate has centered around employment effects, a full accounting of the costs of regulation should properly focus on the effects of regulation on productivity and the bene®ts in health and other outcomes. Related research on South Coast re®neries (Berman and Bui, 1998) has found productivity gains between 1987±92, in contrast to declining productivity in comparison regions. A symmetric analysis of the bene®ts of the South Coast regulations in improved air quality and health outcomes of residents would form the basis for a much more complete economic evaluation of this important and unprecedented episode in air quality regulation.

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Climate Policy and Corporate Behaviour

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Abstract: In this paper, we study the impact of energy taxes and the EU ETS on a large number of firms in Europe between 1996 and 2007. Using company level micro-data, we examine how firms in different sectors were affected by environmental policies. Aspects of behaviour and performance studied include total factor productivity, employment levels, investment behaviour and profitability. On the whole, energy taxes increased total factor productivity and returns to capital but decreased employment, with a mixed effect on investment, for the sectors included in our analysis. However, large sectoral variation is observed, with some industries losing out in terms of productivity and profitability when faced with increased energy taxes, while others benefitted.

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Key words: energy taxes, productivity, investment, firm performance

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Climate Policy and Corporate Behaviour

1. Introduction

With increasing emphasis on environmental regulation in the industrial sector in recent years, it is important to understand the impacts of such measures on firm productivity and investment behaviour. While much attention has been focused on the environmental benefits of differing climate policies, there is relatively little empirical evidence of their impact on company behaviour.

In this paper, we are interested in the effect of energy and carbon taxes on various measures of corporate behaviour and performance. Using firm level micro-data, we focus on the influence of these taxes on the employment levels, investment behaviour and productivity of European companies for the years 1996 to 2007.

Theory provides conflicting guidance as to the likely effects of environmental regulation and taxes on firm behaviour and performance. Taxes represent additional costs for a firm, and as such would be expected to be a constraint on their production possibilities and thus reduce profits. However, when faced with higher environmental taxes, firms may seek to reduce their costs by locating in "pollution havens" or countries where environmental standards or regulatory costs are relatively low. This is known as the pollution haven hypothesis.

Other models stress the importance of the availability of clean natural resources as factor inputs, which could help to improve the production possibilities of firms (factor endowment hypothesis). Equally, technology innovation as a result of increased regulation is also considered a potential outcome. According to the Porter Hypothesis (Porter 1991; Porter & van der Linde 1995), environmental regulation provides incentives for companies to innovate, which can increase competitiveness and productivity. Both the factor endowment and Porter Hypotheses imply that environmental stringency may lead to improvements in the performance of firms as well as advancing environmental goals (Wagner 2003)

There is also some previous empirical research into the impact of environmental regulation on company behaviour and performance. Leiter *et al.* (2009) study firm investment decisions in response to environmental protection measures. Using European industry-level panel data, they find a positive but diminishing impact of environmental stringency on investment. Average elasticities of around 0.15 for industry expenditure on environmental protection and 0.06 for country revenue from environmental taxes are found.

Veith *et al*. (2008) examine the impact of the EU ETS on capital market responses in the power generation sector. Returns on common stock in this sector are found to be positively correlated with rising prices for emissions rights. This indicates that the ETS increases profits, as firms pass on or even overcompensate for regulation costs in prices charged to customers, thus increasing their profitability.

A study undertaken as part of the EU COMETR study, Enevoldsen (2007), includes an analysis of eight sectors in seven European countries. The results show a slightly negative effect of energy taxes on competitiveness and output. However, Henderson and Millimet (2005), using a US sample, find insignificant effects of environmental stringency on state-level output.

While most of the previous literature is undertaken at country or industry level, there has been relatively little research undertaken using firm level micro-data. Anger and Oberndorfer (2008) assess the impact of the EU ETS on firm performance and employment. Using a sample of German firms, they do not find an effect of the relative allocation of emission allowances on firm revenue and employment in 2005. Martin *et al.* (2009) investigate the effect of a UK energy tax, the climate change levy, on the manufacturing sector using firm panel data. However, they find no significant impacts on employment, gross output or total factor productivity (TFP).

Economic theory and previous empirical research suggests conflicting or ambiguous outcomes of environmental policies on corporate performance. The pollution haven hypothesis would suggest decreased employment in more stringently regulated sectors, and the assumption that taxes cause additional cost burdens on firms would equally point to decreased productivity and profitability. However, the Porter Hypothesis and Factor Endowment theory suggest otherwise. They indicate the potential for increased output and TFP due to the availability of clean natural resources, or to increased innovation.

However, we can see from previous empirical work that these findings vary widely, based on which sectors and countries are included in the analysis. The literature finds conflicting or insignificant results for competitiveness, employment and investment amongst different country and industry samples. The type of regulation is also important to take into consideration, as energy taxes are expected to be more efficient than some other forms of regulation, such as command-and-control approaches. For this reason it is important to utilise data at the most disaggregated firm level, and to undertake cross-country and cross-industry analysis in order to examine the validity of these conflicting viewpoints.

In this paper, we make use of cross-country firm level panel data, for a large sample of European companies. In order to test the differing hypotheses, we examine how firms in different industries adapt their structure and behaviour in response to energy taxes and the introduction of the EU Emissions Trading Scheme. We assess the effects of such environmental policies on employment, investment and productivity over a twelve year period.

2. Data

The data employed in this paper is firm-level panel data for a range of European firms across various sectors, provided in the AMADEUS database. This database contains financial and economic information for approximately 11 million firms across Europe. The dataset we used covered the years 1996 to 2007.

From this we construct four dependent variables, representing several measures of corporate performance. These include:

- Total Factor Productivity (TFP)
- Number of employees
- Investment (calculated as change in tangible fixed assets minus depreciation)
- Return on capital employed

While our main data source is the Amadeus database, additional industry and country level variables are collected from a range of sources, such as the OECD, Eurostat and the International Energy Agency.

Energy tax data is sourced from the Eurostat environmental accounts. These consist of taxes on energy products such as petrol, diesel, fuel oils, natural gas, coal and electricity. CO_2 taxes are also included where applicable. Both energy taxes at time t, as well as lagged energy taxes, are incorporated into each model. Firms covered under the EU Emissions Trading Scheme are indicated by a binary variable, given a value of 1 for all sectors included in the scheme since its introduction in 2005, and 0 for all other sectors.

In addition, labour cost shares at country level, calculated as total country labour costs as a proportion of output, are collected from the OECD and included as independent variables in both the employment and TFP models. Other determinants of TFP are also included as controls. Educational attainment at country level (the proportion of people aged 25-64 with a third level education), national output gap, and the import intensity of each industry were obtained from the OECD. Electricity prices per country, from the International Energy Agency, are also included.

We use TFP as our productivity measure since changes in TFP directly reflect efficiency gains due to the reorganization of production processes (Factor Endowment Hypotheses) as well as the introduction of new technologies or innovations related to improvements of a firm's energy efficiency (Porter Hypotheses). We derive TFP of firm j in sector s at time t as a residual from a production function in logs:

$$y_{jst} = \beta_s^k k_{jst} + \beta_s^l l_{jst} + \alpha_i + \eta_s + \mu_t + \varepsilon_{jst} \tag{2}$$

where y_{jst} denotes a firm's real value added, k_{jst} the real physical capital stock and l_{jst} the labour input, α_i is a vector of country specific effects, η_s a vector of industry specific effects, μ_t a vector of year specific effects, $\beta = (\beta^k \beta^l)$ a vector of average input elasticities, and ε_{jst} an error term.[1]

We estimate (2) to obtain empirical measures of the average input elasticities β from firm level data. We account for heterogeneous input elasticities across three-digit (NACE) industry levels in that we estimate the marginal input effects separately for each of the three-digit industries. Note, however, that we pool the observations in each three-digit industry across countries in order to obtain sufficient information for robust production function estimations per industry. We believe that this is a relatively minor restriction on the data since average input elasticities for three-digit industries are typically found to be relatively homogeneous across European countries. Obtaining the estimates for the average input elasticities for each three-digit industry allows us, together with the information on y, k, and l of each individual firm, to compute residual TFP measures at the firm-level. However, the estimation of (2) involves an endogeneity problem which is well-known in the literature on production functions estimation. That is, a firm's demand for labour is expected to depend on its contemporaneous productivity level which is unobserved and hence captured in the error term. In such a case, the estimated input elasticities would be biased. Appropriate instruments for labour services that are uncorrelated with productivity are typically not available. Being aware of this problem, we consistently estimate (2) following Olley and Pakes (1996) who propose a semi-parametric estimator to correct for this simultaneity bias by imposing additional restrictions on the data. In particular, the authors use changes in firm's investment decision as a proxy for the productivity shock. The method supposes that a firm's investment decision is a function of its capital stock, age, and its unobserved productivity. Hence, the unobserved productivity parameter can be modeled as some (inverse) function of investments, capital, and age given the assumption of a monotonic relationship between investment and productivity. We apply this methodology to derive consistent estimates of the average input elasticities in our sample.

Variable definitions and sample means are presented in Tables 2 and 3 below.

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[1] Real variables are obtained deflating by the national output price deflators. Unfortunately, price deflators were not available at the industry level for most of the countries.

3. Methodology

We have estimated four models, each exploring a different aspect of company behaviour or performance. First, our model of employment tests the suggestion that these taxes weaken the incentive to use capital due to high energy-capital complementarity, with firms switching to more labour-intensive activities. However, decreased employment may also be observed in heavily regulated sectors, as firms seek to minimise their costs by moving towards countries or industries with lower levels of stringency. Labour costs are included in this model, to control for differing labour costs across countries over time, which may otherwise be driving the change in a firm's number of employees.

Total factor productivity measures the component of output that arises from factors other than capital and labour. This is often regarded as the impact of technology innovation on firm performance. In this case, energy taxes may have a positive or negative effect, depending on which of the previously outlined theories of environmental regulation are seen to hold. This model controls for additional TFP determinants such as education levels, the gap between actual and potential GDP (output gap), and the import intensity of the specific sector. While import intensity and education, representing higher human capital levels, would be expected to increase TFP, we expect the output gap variable to have a negative sign. Although firms may be expected to innovate and reorganise when operating in a country with an increasing output gap, there may be a loss of knowledge capital in such countries, which tend also to have high unemployment levels. Moreover, some forms of labour input that tend to increase in a capacity-constrained economy (e.g. overtime working) may be omitted from the measure of labour inputs and thereby boost TFP when the output gap is shrinking. On balance, these effects are likely to imply that increasing the output gap will negatively influence firms' TFP levels. Electricity prices are also included and are expected to have a negative effect on TFP.

Return on capital employed is included as a profitability indicator. Energy taxes would be expected to decrease profitability under the assumption that taxes act as an additional costs on doing business. Finally, our fourth company behaviour variable is investment. If energy taxes have similar effects to taxes on capital, there would be an expected negative sign on these coefficients in the investment model, as firms substitute capital for labour. The pollution haven hypothesis also points towards negative effects on investment. However, the Porter Hypothesis would suggest that firms facing increased regulation would have an incentive to innovate and invest in new technology in order to improve productivity. This would suggest increases in investment due to energy taxes. However, it is necessary to empirically examine these in further detail in order to test the competing theoretical stories.

In order to control for unobserved time- and company-specific heterogeneity, we use panel regression analysis. We allow for sectoral variations in energy tax effects by including sector-tax interaction terms for energy tax levels and lagged tax levels.

Many of the variables included exhibit some intertemporal persistence or are non-stationary (e.g. investment, employment), so estimating the models in levels would be expected to lead to substantial residual serial correlation. To avoid this, we estimate the regressions in first differences. The coefficients may thus be viewed as representing equilibrium values.

4. Results

Since the focus of interest for this research is on tax effects, we first present estimates of the tax effects by sector for each model. Later in the section we discuss other explanatory variables.

Tax effects

Sectoral variations in tax effects feature prominently in all four models. These are calculated for each sector by adding the tax coefficients and the tax-sector interaction coefficients for both the current period and lagged taxes. The results for TFP are shown in Figure 1 below. The figure shows the percent change in the TPF growth rate for each sector associated with a 1% tax increase. Thus a 10% tax increase would be associated with a 10% fall in the TFP growth rate for the tobacco sector. If TFP in this sector would otherwise grow by 2%, this implies a lower growth rate of 1.8% due to the tax change.

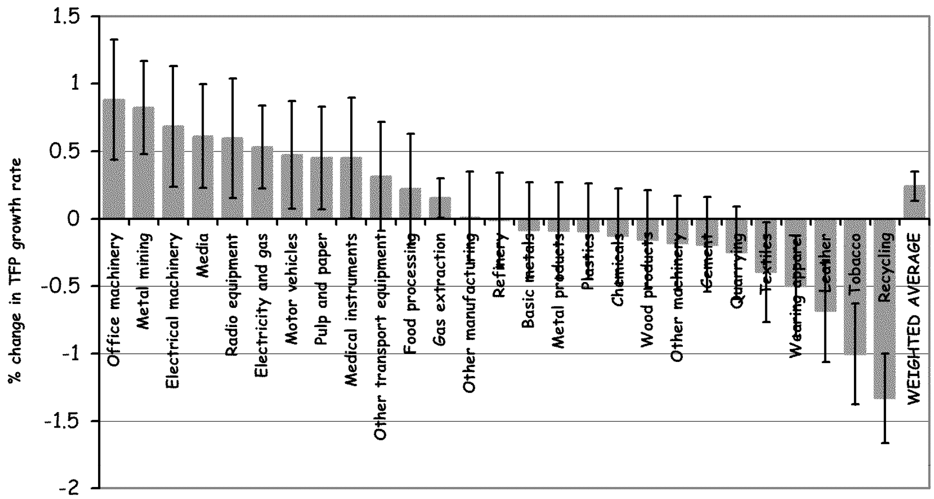

Figure 1: Average partial effect of 1% rise in energy taxes on TFP growth by sector

TFP growth is positively associated with energy taxes in some sectors, but reduced in others. This provides some evidence for Porter Hypothesis effects, but only for selected sectors. Primary resource sectors such as coal, metal, oil and gas extraction benefited from higher TFP growth, along with a range of manufacturing sectors producing energy-using goods (e.g. office machinery, electrical machinery, radio equipment). Electricity and gas generation and the media sector also showed a positive effect. Many sectors showed no statistically significant effect (standard errors were relatively high in this model), but wearing apparel, leather, tobacco and recycling showed a negative association with energy taxes. The average effect of a tax change on TFP growth, weighting sectoral effects by the output shares of these sectors in Europe, is positive.[2] This suggests that *ceteris paribus* a marginal tax increase would lead to a small but statistically significant improvement in TFP growth for these sectors in Europe.

[2] The sector shares were obtained from the OECD STAN database.

The relationship between energy taxes and employment for different sectors is set out in Figure 2 below.

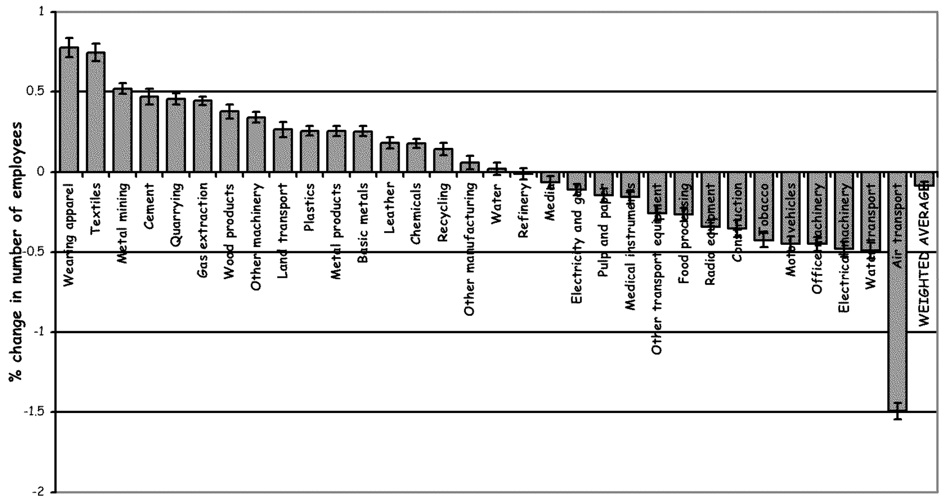

Figure 2: Average partial effect of 1% rise in energy taxes on firms' employment by sector

In this case, standard errors are much lower and most sectors exhibit a significant effect. Some sectors show a positive employment effect relating to energy taxes; notably wearing apparel, textiles, and primary resource sectors. Air transport shows a strongly negative association, with a 10% tax rise being associated with a 15% reduction in employment. Other sectors exhibit weaker positive or negative effects. In this case, the average effect (weighted by sectoral employment shares in Europe) is negative. Overall, then, a marginal increase in energy taxes is associated with lower employment for this set of sectors in Europe.

Air transport also features a large and significant effect in relation to corporate investment (Figure 3 below). In this case the effect is positive, with a hypothetical 10% tax rise being associated with a 20% increase in fixed investment. Basic metals, refining and water transport also have relatively large positive coefficients, while tobacco has a very large negative association and the recycling and leather sectors have smaller negative coefficients. The average effect, weighted by total sectoral investment, is not significantly different from zero. This implies that energy taxes at the margin do not have a statistically significant effect on total investment levels in this sample.

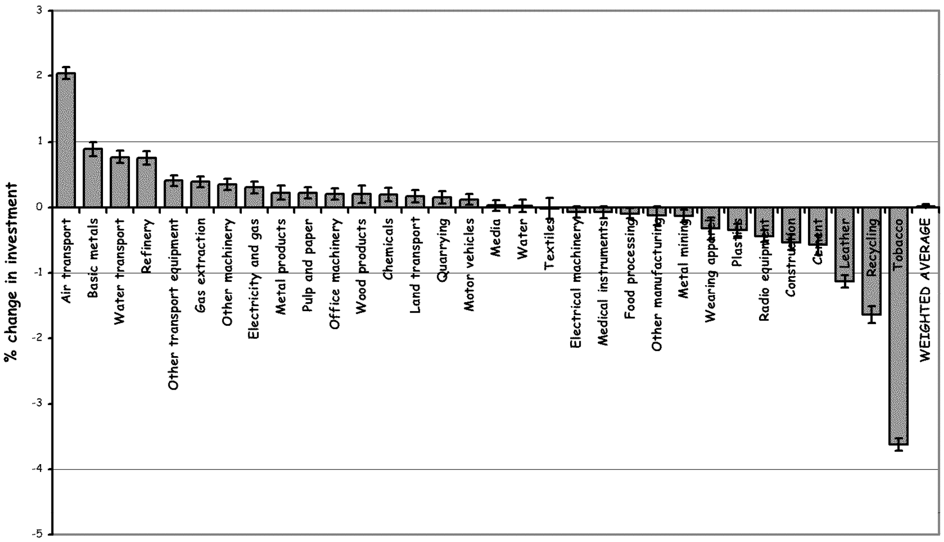

Figure 3: Average partial effect of 1% rise in energy taxes on firms' investment by sector

Our final model examined the association between energy taxes and company profitability, proxied by the return on capital employed. This relationship proves to be positive in most cases, with the strongest relationship being for air transport. Only a few sectors – water transport, refining, wood products, coal and peat extraction, food processing and quarrying having significant negative coefficients. The average effect, weighting sectoral effects by the output shares of these sectors in Europe, is positive and statistically significant. This suggests that a marginal increase in energy taxes would increase profitability on average.

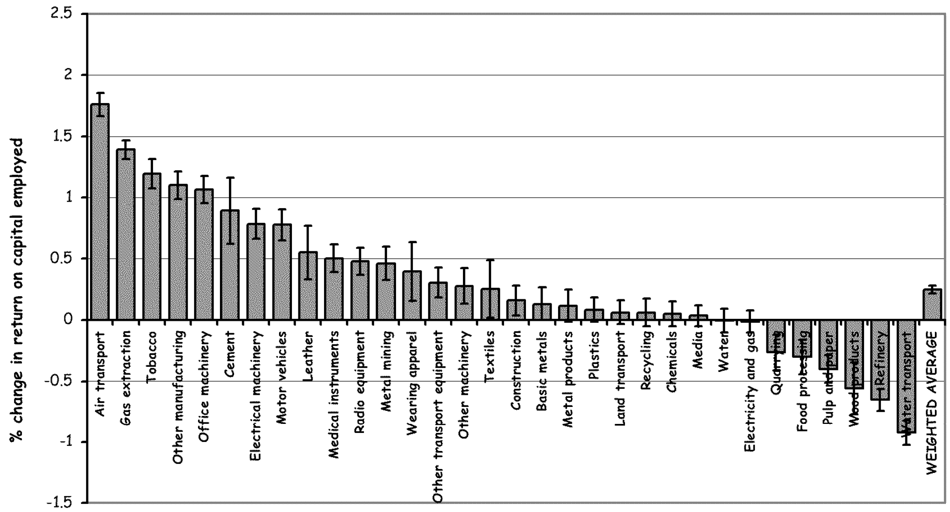

Figure 4: Average partial effect of 1% rise in energy taxes on firms' return on capital employed by sector

We considered whether the sectoral pattern of energy tax effects shown above might be driven by broader sectoral characteristics such as energy intensity or technology intensity. However, grouping sectors by these classifications did not reveal any obvious association with the tax effects. The impact of energy taxes on TFP, employment, investment and profitability vary by sector even amongst industries which have similar energy and technology use.

Other effects

Since we have estimated these models in differences, we only observe effects for factors that vary over time. All models allow for ETS participation effects, and the relevant coefficients are shown in Table 1 below. We find no significant association between ETS participation and employment or investment. However, both TFP growth and return on capital employed were lower in ETS participant firms, *ceteris paribus*. For a firm with a TFP growth rate of 2%, participation in the ETS would be associated with 0.12% lower TFP growth.

Table 1: ETS participation effects

Dependent variable	ETS effect	Robust standard error
TFP growth	-0.0616***	0.0196
ln(employment)	0.0173	0.0142
ln(investment)	0.00161	0.0249
ln(Return on capital employed)	-0.0673***	0.0185

Lower productivity and profitability among ETS firms is consistent with the view that the scheme increased firms' costs without inducing significant Porter Hypothesis effects. With the dataset we are using here, it is not possible to tell whether a different design or level of stringency for the ETS would have changed this conclusion.

Finally, we can report a range of secondary results. In the TFP model, sectoral import intensity, national education level and labour costs were not significant (we had expected the first two factors to have a positive effect on TFP and the third to have a negative effect). Labour cost was, as expected, negative and highly significant in the employment model. Returning to the TFP model, the output gap and electricity prices both showed highly significant negative effects, which was in line with our expectations.

5. Conclusions

In this paper, we study the impact of energy taxes and the EU ETS on a large number of firms in Europe between 1996 and 2007. To the best of our knowledge, we are the first to do so. We estimate the effect on the change in total factor productivity (a proxy for technological progress), on employment, on investment, and on the returns to capital (a proxy for accounting profits). The following results emerge. First, as one would expect, results vary dramatically between sectors, not just in the size of the estimated effects but also in their signs. Second, total factor productivity accelerates with higher carbon taxes. Although the effect is insignificant in large parts of the economy, and negative in some sectors, the positive impact in a number of sectors dominates. This finding supports the Porter Hypothesis. Regulation spurs innovation. Third, energy taxes reduce employment. There is a significant impact on employment in almost all sectors. The most important effect is a large shift in labour between sectors, but the overall effect is negative. While energy taxes create jobs, more jobs are destroyed. Fourth, energy taxes increase investment. The impact is again significant in most sectors, and the most notable effect is a shift in investment between sectors. The aggregate effect is positive, however. This suggests that businesses respond to energy taxes by substituting labour for capital. This is in sharp contrast to the findings by Koetse et al. (2008). Fifth, energy taxes increase the returns to capital. Again, differences between sectors are pronounced, but the average effect is positive. This finding reinforces the results for investment.

We obtain different results for the EU ETS. The effect on productivity and profits are negative, while the effect on labour and investment are insignificant. These results are indicative only, as our data only cover the experimental phase of the ETS and we were unable to define a permit price. Future research, using data from the second phase of the ETS, should reinvestigate this.

Acknowledgments

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Tables

Table 2: Variable definitions

Dependent variables		**Independent variables**	
Total Factor Productivity (lnTFP)	Olley-Pakes method. Log TFP in first differences	Tax rate (lntax_rate)	Log of energy taxes by sector and country, first differences. Includes taxes on petrol, diesel, gas, electricity etc.
Employment (lnL)	Log number of employees in a firm in year t, in first differences.	Lagged Tax rate (lntax_rate t-1)	Log of energy taxes, 1 period lag. First differences.
Return on Capital Employed	Return on capital employment in year t, in first differences	Import Intensity	Imports/ (Production – Exports + Imports)
Investment	Log change in tangible fixed assets minus depreciation, in first differences	Education	Tertiary education attainment for age group 25-64, as a percentage of the population of that age group in each country.
		Output Gap	Deviations of actual GDP from potential GDP as a percentage of potential GDP
		Electricity price (ln elec price)	Electricity prices per country (€ per kWH)
		Labour Cost	Total Labour Costs as a percentage of Output, per country
		ETS	Emissions Trading Scheme dummy variable, 1 if sector covered by ETS, 0 otherwise

Table 3: Variable means

	TFP	**Employment**	**Return On Capital Employed**	**Investment**
Independent Variables	227942 obs.	649809 obs.	506682 obs.	427483 obs.
Tax rate	0.004	0.003	0.004	0.004
Lagged Tax rate	0.004	0.003	0.004	0.004
Labour Cost	0.589	0.623		
Education	19.742			
Output Gap	-0.093			
Electricity price	0.070			
Import Intensity	0.277			

Annex A: Regression results

Table 4: Total factor productivity regression results, OLS panel regression in first differences; dependent variable: ln(TFP_{it})

Variables and statistics	Coef.	Robust standard error
Ltax_rate	0.935***	0.109
Ltax_rate-1	0.257***	0.0539
import_intensity	0.0268	0.0228
education	-0.0552	0.0331
output_gap	-0.166***	0.0568
Lelectricityprice	-0.129***	0.0382
labourcost	-1.77	2.9
ETS	-0.0616***	0.0196
NACExTax11	-1.04***	0.00638
NACExTax13	-0.371***	0.104
NACExTax14	-1***	0.104
NACExTax15	-1.01***	0.0808
NACExTax16	-2.95***	0.0588
NACExTax17	-1.04***	0.121
NACExTax18	-1.09***	0.12
NACExTax19	-1.49***	0.127
NACExTax20	-0.956***	0.118
NACExTax21	-0.79***	0.0732
NACExTax22	-0.63***	0.0751
NACExTax23	-0.823***	0.106
NACExTax24	-0.956***	0.108
NACExTax25	-0.945***	0.109
NACExTax26	-0.97***	0.108
NACExTax27	-0.924***	0.11
NACExTax28	-0.926***	0.111
NACExTax29	-0.963***	0.107
NACExTax30	-0.795***	0.112
NACExTax31	-0.631***	0.115
NACExTax32	-0.782***	0.111
NACExTax33	-0.875***	0.116
NACExTax34	-0.804***	0.0696
NACExTax35	-0.688***	0.071

Variables and statistics	Coef.	Robust standard error
NACExTax36	-0.974***	0.0966
NACExTax37	-1.72***	0.09
NACExTax40	-0.655***	0.0765
NACExTax14	-0.443***	0.0117
NACExTax15	0.0366	0.266
NACExTax16	0.753***	0.258
NACExTax17	-0.554***	0.0529
NACExTax18	-0.597***	0.0528
NACExTax19	-0.391***	0.0374
NACExTax20	-0.391***	0.0383
NACExTax21	0.0464	0.235
NACExTax22	0.0475	0.236
NACExTax23	-0.378***	0.0417
NACExTax24	-0.363***	0.0317
NACExTax25	-0.34***	0.0257
NACExTax26	-0.413***	0.0495
NACExTax27	-0.351***	0.0241
NACExTax28	-0.355***	0.0236
NACExTax29	-0.407***	0.0268
NACExTax30	0.482*	0.266
NACExTax31	0.121	0.262
NACExTax32	0.185	0.261
NACExTax33	0.131	0.26
NACExTax34	0.0835	0.273
NACExTax35	-0.191	0.275
NACExTax36	-0.211***	0.0747
NACExTax37	-0.802***	0.0677
NACExTax40	-0.00815	0.0479
D1998	0.378***	0.0983
D1999	0.363***	0.103
D2000	0.359***	0.117
D2001	0.168***	0.0498
D2003	-0.0209*	0.0103
D2004	0.178***	0.0634
D2005	-0.0453*	0.0232
Constant	-0.0438**	0.016

Variables and statistics	Coef.	Robust standard error
Sample	65,787 firms	
Observations	227,942	
Min. periods	1	
Avg. periods	3.5	
Max. periods	7	
R^2 within	0.0156	
R^2 between	0.0004	
R^2 overall	0.0077	
*Note: All variables are in first differences apart from the constant, and variables with an L prefix are in log terms. *, ** and *** denote significant at the 10%, 5% and 1% level respectively. t-statistics are heteroscedasticity-robust and allow for clustering at sector level.*		

Table 5: Employment regression results, OLS panel regression in first differences, dependent variable: ln(employment$_{it}$)

Variables and statistics	Coef.	Robust standard error
Ltax_rate	0.299***	0.0234
Ltax_rate-1	-0.062***	0.0113
labourcost	-2.29***	0.296
ETS	0.0173	0.0142
NACExTax11	-0.294***	0.00741
NACExTax13	-0.172***	0.013
NACExTax14	-0.0279	0.0178
NACExTax15	-0.328***	0.0242
NACExTax16	0.434***	0.0242
NACExTax17	0.0702**	0.0333
NACExTax18	0.0421	0.0368
NACExTax19	-0.162***	0.0127
NACExTax20	-0.224***	0.014
NACExTax21	-0.297***	0.0223
NACExTax22	-0.35***	0.0236
NACExTax23	-0.329***	0.021
NACExTax24	-0.222***	0.00984
NACExTax25	-0.168***	0.00964
NACExTax26	-0.0538*	0.0301
NACExTax27	-0.199***	0.00909
NACExTax28	-0.211***	0.00932
NACExTax29	-0.171***	0.0112
NACExTax30	-0.504***	0.0206
NACExTax31	-0.648***	0.0221
NACExTax32	-0.671***	0.0198
NACExTax33	-0.429***	0.0205
NACExTax34	-0.616***	0.0255
NACExTax35	-0.379***	0.0265
NACExTax36	-0.29***	0.0246
NACExTax37	-0.315***	0.0228
NACExTax40	-0.358***	0.0218
NACExTax41	-0.355***	0.0239
NACExTax45	-0.583***	0.0308
NACExTax60	-0.271***	0.0287
NACExTax61	-0.659***	0.029

Variables and statistics	Coef.	Robust standard error
NACExTax62	-0.861***	0.0299
NACExTax11	0.501***	0.00686
NACExTax13	0.455***	0.0167
NACExTax14	0.246***	0.0158
NACExTax15	-0.172***	0.0237
NACExTax16	-1.1***	0.0231
NACExTax17	0.439***	0.0382
NACExTax18	0.5***	0.0395
NACExTax19	0.106***	0.0166
NACExTax20	0.364***	0.0301
NACExTax21	-0.0845***	0.0229
NACExTax22	0.0489***	0.0148
NACExTax23	0.0806***	0.00946
NACExTax24	0.163***	0.00866
NACExTax25	0.189***	0.00898
NACExTax26	0.287***	0.0274
NACExTax27	0.216***	0.0132
NACExTax28	0.23***	0.0129
NACExTax29	0.275***	0.0139
NACExTax30	-0.18***	0.0276
NACExTax31	-0.0673**	0.0297
NACExTax32	0.0927***	0.027
NACExTax33	0.0369	0.0272
NACExTax34	-0.0669**	0.0284
NACExTax35	-0.112***	0.0293
NACExTax36	0.113***	0.0245
NACExTax37	0.222***	0.0175
NACExTax40	0.0121	0.0126
NACExTax41	0.139***	0.0101
NACExTax45	-0.00434	0.0342
NACExTax60	0.299***	0.0304
NACExTax61	-0.0668**	0.031
NACExTax62	-0.868***	0.0319
D1998	0.101	0.0128
D1999	0.0445	0.0129
D2000	0.147	0.0115

Variables and statistics	Coef.	Robust standard error
D2001	0.144	0.0122
D2002	0.0656	0.00897
D2003	0.0386	0.0113
D2004	-0.014	0.0131
Constant	-0.0115	0.00956
Sample	164,570 firms	
Observations	649,809	
Min. periods	1	
Avg. periods	3.9	
Max. periods	8	
R^2 within	0.0160	
R^2 between	0.0073	
R^2 overall	0.0048	

*Note: All variables are in first differences apart from the constant, and variables with an L prefix are in log terms. *, ** and *** denote significant at the 10%, 5% and 1% level respectively. t-statistics are heteroscedasticity-robust and allow for clustering at sector level.*

Table 6: Return on capital employed, OLS panel regression in first differences, dependent variable: ln($ROCE_{it}$)

Variables and statistics	Coef.	Robust standard error
Ltax_rate	-0.155***	0.0427
Ltax_rate-1	-0.298***	0.0486
ETS	-0.0673***	0.0185
NACExTax11	0.375***	0.026
NACExTax13	-1.37***	0.0308
NACExTax14	-0.224***	0.0673
NACExTax15	0.512***	0.0981
NACExTax16	0.766***	0.0849
NACExTax17	-0.289***	0.105
NACExTax18	-0.298**	0.112
NACExTax19	0.2	0.127
NACExTax20	-0.337**	0.136
NACExTax21	0.157***	0.0419
NACExTax22	0.342***	0.0305
NACExTax23	-0.162***	0.057
NACExTax24	-0.0174	0.0634
NACExTax25	-0.0315	0.0598
NACExTax26	-0.0686	0.13
NACExTax27	-0.0535	0.0882
NACExTax28	-0.0356	0.0843
NACExTax29	0.129*	0.0737
NACExTax30	0.416***	0.0599
NACExTax31	0.715***	0.0754
NACExTax32	0.396***	0.062
NACExTax33	0.295***	0.0598
NACExTax34	0.611***	0.0592
NACExTax35	0.404***	0.054
NACExTax36	0.98***	0.0477
NACExTax37	0.649***	0.08
NACExTax40	0.267***	0.0534
NACExTax41	0.147***	0.0534
NACExTax45	-0.000523	0.0556
NACExTax60	0.514***	0.0528
NACExTax61	0.0725	0.058
NACExTax62	1.2***	0.047

Variables and statistics	Coef.	Robust standard error
NACExTax11	1.47***	0.0314
NACExTax13	2.29***	0.113
NACExTax14	0.412***	0.111
NACExTax15	-0.362***	0.0748
NACExTax16	0.884***	0.0519
NACExTax17	0.995***	0.2
NACExTax18	1.14***	0.204
NACExTax19	0.801***	0.166
NACExTax20	0.23	0.147
NACExTax21	-0.106	0.0825
NACExTax22	0.144***	0.0505
NACExTax23	-0.0366	0.0271
NACExTax24	0.518***	0.0446
NACExTax25	0.569***	0.0483
NACExTax26	1.41***	0.229
NACExTax27	0.637***	0.0758
NACExTax28	0.605***	0.0785
NACExTax29	0.6***	0.106
NACExTax30	1.1***	0.0695
NACExTax31	0.523***	0.0703
NACExTax32	0.533***	0.0662
NACExTax33	0.66***	0.0672
NACExTax34	0.619***	0.0923
NACExTax35	0.352***	0.0872
NACExTax36	0.577***	0.0775
NACExTax37	-0.135***	0.0415
NACExTax40	0.171***	0.0354
NACExTax41	0.299***	0.0474
NACExTax45	0.611***	0.0894
NACExTax60	-0.000539	0.0508
NACExTax61	-0.542***	0.0525
NACExTax62	1.02***	0.0528
D1999	-0.404***	0.0918
D2000	-0.21***	0.0505
D2001	-0.213***	0.0296
D2002	-0.251***	0.0355

Variables and statistics	Coef.	Robust standard error
D2003	-0.292***	0.0411
D2004	-0.23***	0.0463
D2005	-0.236***	0.0465
Constant	0.15***	0.0432
Sample	162,771 firms	
Observations	506,682	
Min. periods	1	
Avg. periods	3.1	
Max. periods	8	
R^2 within	0.0082	
R^2 between	0.0027	
R^2 overall	0.0064	

*Note: All variables are in first differences apart from the constant, and variables with an L prefix are in log terms. *, ** and *** denote significant at the 10%, 5% and 1% level respectively. t-statistics are heteroscedasticity-robust and allow for clustering at sector level.*

Table 7: Investment, OLS panel regression in first differences, dependent variable: ln(investment$_{it}$)

Variables and statistics	Coef.	Robust standard error
Ltax_rate	-1.68***	0.0303
Ltax_rate-1	1.92***	0.0554
ETS	0.00161	0.0249
NACExTax11	1.93***	0.0257
NACExTax13	-0.225***	0.0469
NACExTax14	1.65***	0.0535
NACExTax15	1.73***	0.0296
NACExTax16	1.45***	0.0498
NACExTax17	1.68***	0.103
NACExTax18	1.13***	0.115
NACExTax19	1.27***	0.0325
NACExTax20	1.9***	0.0954
NACExTax21	1.67***	0.0385
NACExTax22	1.73***	0.0305
NACExTax23	2.37***	0.0364
NACExTax24	1.93***	0.048
NACExTax25	1.38***	0.0404
NACExTax26	0.847***	0.129
NACExTax27	2.43***	0.0586
NACExTax28	1.87***	0.0544
NACExTax29	1.85***	0.0622
NACExTax30	1.31***	0.0383
NACExTax31	1.49***	0.0376
NACExTax32	1.14***	0.0398
NACExTax33	1.31***	0.041
NACExTax34	2***	0.0346
NACExTax35	1.94***	0.0374
NACExTax36	1.49***	0.0911
NACExTax37	0.307***	0.0725
NACExTax40	1.95***	0.0301
NACExTax41	1.75***	0.0311
NACExTax45	1.26***	0.0627
NACExTax60	1.45***	0.0493
NACExTax61	1.41***	0.0498
NACExTax62	2.62***	0.0446

Variables and statistics	Coef.	Robust standard error
NACExTax11	-1.78***	0.039
NACExTax13	-0.141***	0.0462
NACExTax14	-1.73***	0.0353
NACExTax15	-2.05***	0.037
NACExTax16	-5.31***	0.0466
NACExTax17	-1.93***	0.105
NACExTax18	-1.69***	0.105
NACExTax19	-2.63***	0.0615
NACExTax20	-1.93***	0.0713
NACExTax21	-1.68***	0.044
NACExTax22	-1.94***	0.0431
NACExTax23	-1.85***	0.0669
NACExTax24	-1.97***	0.0595
NACExTax25	-1.96***	0.0615
NACExTax26	-1.65***	0.0461
NACExTax27	-1.78***	0.065
NACExTax28	-1.88***	0.0622
NACExTax29	-1.73***	0.0165
NACExTax30	-1.34***	0.0467
NACExTax31	-1.8***	0.0457
NACExTax32	-1.81***	0.0481
NACExTax33	-1.61***	0.0498
NACExTax34	-2.11***	0.0368
NACExTax35	-1.77***	0.037
NACExTax36	-1.85***	0.0966
NACExTax37	-2.18***	0.0861
NACExTax40	-1.88***	0.0554
NACExTax41	-1.96***	0.0556
NACExTax45	-2.03***	0.0796
NACExTax60	-1.52***	0.0488
NACExTax61	-0.877***	0.0507
NACExTax62	-0.802***	0.0533
D1999	-0.0204	0.0395
D2000	-0.0842**	0.0412
D2001	-0.196***	0.0368
D2002	-0.113**	0.0468

Variables and statistics	Coef.	Robust standard error
D2003	-0.151***	0.0354
D2004	-0.074*	0.037
D2005	-0.148***	0.0367
Constant	0.166***	0.0371
Sample	138,776 firms	
Observations	427,483	
Min. periods	1	
Avg. periods	3.1	
Max. periods	8	
R^2 within	0.0017	
R^2 between	0.0002	
R^2 overall	0.0012	

*Note: All variables are in first differences apart from the constant, and variables with an L prefix are in log terms. *, ** and *** denote significant at the 10%, 5% and 1% level respectively. t-statistics are heteroscedasticity-robust and allow for clustering at sector level.*

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MODERN LABOR ECONOMICS

Theory and Public Policy

SEVENTH EDITION

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4

Labor Demand Elasticities

IN 1995 A HEATED DEBATE broke out among economists and policymakers about the employment effects of minimum wage laws. Clearly, the standard theory developed in chapter 3 predicts that if wages are raised above their market level by a minimum wage law, employment opportunities will be reduced as firms move up (and to the left) along their labor demand curves. Two prominent labor economists, however, after reviewing previous work on the subject and doing new studies of their own, published a 1995 book in which they concluded that the predicted job losses associated with increases in the minimum wage simply could not be observed to occur, at least with any regularity.[1] On one level, the findings in this book raised a controversy about the usefulness of standard labor demand theory. Those who found the book and related research persuasive called for the use of new labor demand models (especially ones that are monopsony-like in character), while others argued that the new studies in this book were flawed and confidently asserted that appropriately executed studies would yield the results predicted by standard theory.[2]

On another level, however, the 1995 book simply triggered a highly charged discussion of a long-standing question: just how responsive is employment demand to given changes in wages? Hardly anyone doubts that jobs would be lost if mandated

[1]David Card and Alan B. Krueger, *Myth and Measurement: The New Economics of the Minimum Wage* (Princeton: Princeton University Press, 1995).
[2]Six reviews of Card and Krueger, *Myth and Measurement*, appear in the book review section of the July 1995 issue of *Industrial and Labor Relations Review* 48, no. 4. These reviews give an excellent overview of the range of responses to the Card and Krueger book.

wage increases were huge, but how many are lost with modest increases? One economist framed the issue in this way:

> Economists . . . are divided into two basic groups. On one side are those who believe that responses to price incentives are usually large—the Big Responders (BRs). On the other side are those who believe that responses to price incentives are generally small—Small Responders (SRs). . . . Logic tells us that massive changes in prices . . . will have large effects on quantities. . . . But *whether the BR or SR perspective applies to minimum wages in the range observed in the United States is a purely empirical question.*[3]

The focus of this chapter is on the degree to which employment responds to changes in wages. Chapter 3 examined theory underlying the general nature of labor demand curves. In the context of minimum wages, for example, its major contribution was in helping us understand why we expect at least some job loss if wages are increased above market levels. In contrast, chapter 4 will examine issues concerning the magnitude of the job loss. Put in the context of the above quotation, this chapter will analyze both theory and evidence in the Big Responder–Small Responder debate.

The responsiveness of labor demand to a change in wage rates is normally measured as an "elasticity," which is the percentage change in employment brought about by a 1 percent change in wages. We begin our analysis by defining, analyzing, and measuring "own-wage" and "cross-wage" elasticities. We then apply these concepts to analyses of minimum wage laws and the employment effects of technological innovations. (Because the effects of free trade on the demand for labor are qualitatively similar to those of technological change, we analyze the employment effects of free trade in the appendix to this chapter.)

THE OWN-WAGE ELASTICITY OF DEMAND

The *own-wage elasticity of demand* for a category of labor is defined as the percentage change in its employment (E) induced by a 1 percent increase in its wage rate (W):

$$\eta_{ii} = \frac{\%\Delta E_i}{\%\Delta W_i} \tag{4.1}$$

In equation (4.1), we have used the subscript i to denote category of labor i, the Greek letter η (eta) to represent elasticity, and the notation $\%\Delta$ to represent "percentage change in." Since the previous chapter showed that labor demand curves slope downward, an increase in the wage rate will cause employment to decrease; the own-wage elasticity of demand is therefore a negative number. What is at issue is its magnitude. The larger its *absolute* value (its magnitude, ignoring its sign), the larger will be the percentage decline in employment associated with any given percentage increase in wages.

Labor economists often focus on whether the absolute value of the elasticity of demand for labor is greater than or less than 1. If it is greater than 1, a 1 percent

[3] Richard Freeman, "Comment," *Industrial and Labor Relations Review* 48, no. 4 (July 1995): 830–831.

FIGURE
Relative

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increase in wages will lead to an employment decline of greater than 1 percent; this situation is referred to as an *elastic* demand curve. In contrast, if the absolute value is less than 1, the demand curve is said to be *inelastic:* a 1 percent increase in wages will lead to a proportionately smaller decline in employment. If demand is elastic, aggregate earnings (defined here as the wage rate times the employment level) of individuals in the category will decline when the wage rate increases, because employment falls at a faster rate than wages rise. Conversely, if demand is inelastic, aggregate earnings will increase when the wage rate is increased. If the elasticity just equals −1, the demand curve is said to be *unitary elastic,* and aggregate earnings will remain unchanged if wages increase.

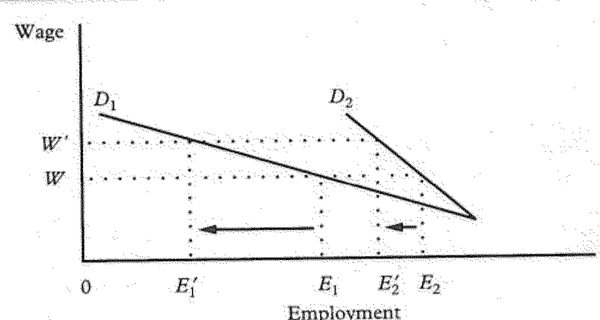
Figure 4.1 shows that the flatter of the two demand curves graphed (D_1) has greater elasticity than the steeper (D_2). Beginning with any wage (W, for example), a given wage change (to W', say) will yield greater responses in employment with demand curve D_1 than with D_2. To judge the different elasticities of response brought about by the same percentage wage increase, compare $(E_1 - E'_1)/E_1$ with $(E_2 - E'_2)/E_2$. Clearly, the more elastic response occurs along D_1.

To speak of a demand curve as having "an" elasticity, however, is technically incorrect. Given demand curves will generally have elastic and inelastic ranges—and while we are usually just interested in the elasticity of demand in the range around the current wage rate in any market, one cannot fully understand elasticity without understanding that it can vary along a given demand curve.

To illustrate, suppose we examine the typical straight-line demand curve that we have used so often in chapters 2 and 3 (see Figure 4.2). One feature of a straight-line demand curve is that, at *each* point along the curve, a unit change in wages induces the *same* response in terms of units of employment. For example, at any point along the demand curve shown in Figure 4.2, a $2 decrease in wages will increase employment by 10 workers.

However, the same responses in terms of *unit* changes along the demand curve do *not* imply equal *percentage* changes. To see this point, look first at the upper end of the demand curve in Figure 4.2 (the end where wages are high and employment is low). A $2 decrease in wages when the base is $12 represents a 17 percent reduction in wages, while an addition of 10 workers when the starting point is also 10 represents a 100 percent increase in demand. Demand at this point is clearly *elastic.*

FIGURE 4.1
Relative Demand Elasticities



However, if one looks at the same unit changes in the lower region of the demand curve (low wages, high employment), demand there is inelastic. A $2 reduction in wages from a $4 base is a 50 percent reduction, while an increase of 10 workers from a base of 50 is only a 20 percent increase. Since the percentage increase in employment is smaller than the percentage decrease in wages, demand is seen to be inelastic at this end of the curve.

Thus, the upper end of a straight-line demand curve will exhibit greater elasticity than the lower end. Moreover, a straight-line demand curve will actually be elastic in some ranges and inelastic in others (as shown in Figure 4.2).

The Hicks-Marshall Laws of Derived Demand

Knowledge of own-wage elasticities of demand is very important for making policy decisions. The factors that influence own-wage elasticity can be summarized by the Hicks-Marshall laws of derived demand—four "laws" named after the two distinguished British economists, John Hicks and Alfred Marshall, who are closely associated with their development.[4] These laws assert that, other things equal, the own-wage elasticity of demand for a category of labor is high under the following conditions:

1. When the price elasticity of demand for the product being produced is high;
2. When other factors of production can be easily substituted for the category of labor;
3. When the supply of other factors of production is highly elastic (that is, usage of other factors of production can be increased without substantially increasing their prices); and
4. When the cost of employing the category of labor is a large share of the total costs of production.

Not only are these laws generally valid as an empirical proposition, but the first three can be shown to always hold. There are conditions, however, under which the final law does not hold.

In seeking to explain why these laws hold, it is useful to act as if we could divide the process by which an increase in the wage rate affects the demand for labor into two steps: First, an increase in the wage rate increases the relative cost of the category of labor in question and induces employers to use less of it and more of other inputs (the *substitution effect*). Second, when the wage increase causes the marginal costs of production to rise, there are pressures to increase product prices and reduce output, causing a fall in employment (the *scale effect*). The four laws of derived demand each deal with substitution or scale effects.

Demand for the Final Product We noted above that wage increases cause production costs to rise and tend to result in product price increases. The greater

[4]John R. Hicks, *The Theory of Wages*, 2d ed. (New York: St. Martin's Press, 1966), 241–247, and Alfred Marshall, *Principles of Economics*, 8th ed. (London: Macmillan, 1923), 518–538.

FIGU

Differe

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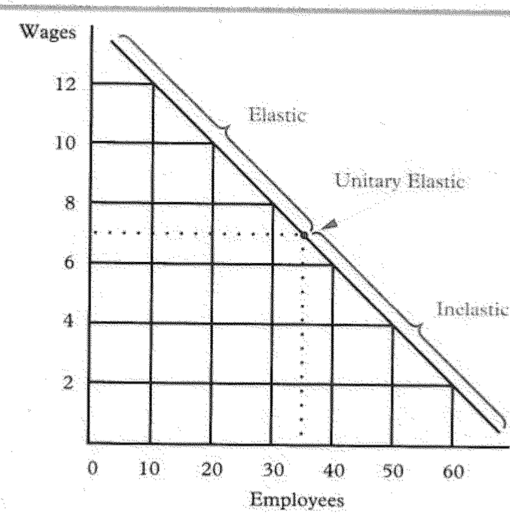
FIGURE 4.2
Different Elasticities along a Demand Curve



the price elasticity of demand for the final product, the larger will be the decline in output associated with a given increase in price—and the greater the decrease in output, the greater the loss in employment (other things equal). Thus, *the greater the elasticity of demand for the product, the greater the elasticity of demand for labor will be.* One implication of this result is that, other things equal, the demand for labor at the *firm* level will be more elastic than the demand for labor at the *industry*, or market, level. For example, the product demand curves facing *individual* carpet-manufacturing companies are highly elastic because the carpet of company X is a very close substitute for the carpet of company Y. Compared to price increases at the *firm* level, however, price increases at the *industry* level will not have as large an effect on demand because the closest substitutes for carpeting are hardwood, ceramic, or some kind of vinyl floor covering—none a very close substitute for carpeting. The demand for labor is thus much more elastic for an individual carpet-manufacturing firm than for the carpet-manufacturing industry as a whole. (For the same reasons, the labor demand curve for a monopolist is less elastic than for an individual *firm* in a competitive industry. Monopolists, after all, face *market* demand curves for their product because they are the only seller in the particular market.)

Another implication of this first law is that *wage elasticities will be higher in the long run than in the short run.* The reason for this is that price elasticities of demand in product markets are higher in the long run. In the short run there may be no good substitutes for a product, or consumers may be locked into their current stock of consumer durables. After a period of time, however, new products that are substitutes may be introduced and consumers will begin to replace durables that have worn out.

Substitutability of Other Factors As the wage rate of a category of labor increases, firms have an incentive to try to substitute other, now relatively cheaper, inputs for the category. Suppose, however, that there were no substitution possibilities; a given number of units of the type of labor *must* be used to produce one unit of output. In this case, there is no reduction in employment due to the substitution effect. In contrast, when substitution possibilities do present themselves, a reduction in employment owing to the substitution effect will accompany whatever reductions are caused by the scale effect. Hence, other things equal, *the easier it is to substitute other factors of production, the higher the wage elasticity of labor demand will be.*

Limitations on substitution possibilities need not be solely technical ones. For example, as we shall see in chapter 13, unions often try to limit substitution possibilities by including specific work rules in their contracts (e.g., minimum crew size for railroad locomotives). Alternatively, the government may legislate limitations by specifying minimum employment levels for safety reasons (for example, each public swimming pool in New York State must always have a lifeguard present). Such collectively bargained or legislated restrictions make the demand for labor less elastic. Note, however, that substitution possibilities that are not feasible in the short run may well become feasible over longer periods of time, when employers are free to vary their capital stock. For example, if the wages of railroad workers went up, companies could buy more powerful locomotives and operate with larger trains and fewer locomotives. Likewise, if the wages of lifeguards rose, cities might build larger, but fewer, swimming pools. Both adjustments would occur only in the long run, which is another reason why the demand for labor is more elastic in the long run than in the short run.

The Supply of Other Factors Suppose that, as the wage rate increased and employers attempted to substitute other factors of production for labor, the prices of these inputs were bid up substantially. This situation might occur, for example, if one were trying to substitute capital equipment for labor. If producers of capital equipment were already operating their plants near capacity, so that taking on new orders would cause them substantial increases in costs because they would have to work their employees overtime and pay them a wage premium, they would accept new orders only if they could charge a higher price for their equipment. Such a price increase would dampen firms' "appetites" for capital and thus limit the substitution of capital for labor.

For another example, suppose an increase in the wages of unskilled workers caused employers to attempt to substitute skilled employees for unskilled employees. If there were only a fixed number of skilled workers in an area, their wages would be bid up by employers. As in the prior example, the incentive to substitute alternative factors would be reduced, and the reduction in unskilled employment due to the substitution effect would be smaller. In contrast, if the prices of other inputs did not increase when employers attempted to increase their usage, other things equal, the substitution effect—and thus the wage elasticity of demand—would be larger.

Note again that prices of other inputs are less likely to be bid up in the long run than in the short run. In the long run, existing producers of capital equipment can expand their capacity and new producers can enter the market. Similarly, in the long run more skilled workers can be trained. This observation is an additional reason why the demand for labor will be more elastic in the long run.

The Share of Labor in Total Costs Finally, the share of the category of labor in total costs is crucial to the size of the elasticity of labor demand. If the category's initial share were 20 percent, a 10 percent increase in the wage rate, other things equal, would raise total costs by 2 percent. In contrast, if its initial share were 80 percent, a 10 percent increase in the wage rate would increase total costs by 8 percent. Since employers would have to increase their product prices by more in the latter case, output, and hence employment, would fall more in that case. *Thus, the greater the category's share in total costs, the higher the wage elasticity of demand will tend to be.*

The discussion of this law, however, has ignored the ease of substituting other factors when the category's cost increases. *An exception to the law occurs when it is easier for employers to substitute other factors of production for the category of labor than it is for customers to substitute other products for the product being produced*; in this case the law is reversed. An example illustrates this exception.[5]

Suppose we classify the carpenters who build houses by their race/ethnicity. For example, we might divide carpenters into African-, Asian-, German-, Hispanic-, Irish-, Italian-, and Polish-American carpenters. Suppose further that carpenters from each group are equally productive and thus that they are perfect substitutes for each other. Finally, suppose that a fixed number of carpenters is required to build each house.

Since the wages of any one subgroup of carpenters would be a small fraction of the aggregate wages paid to *all* carpenters, if the last law always held it would lead one to believe that the wage elasticity of any one group of carpenters would be less than that of all carpenters as a group. This conclusion would be incorrect, however, because if any one group's wages rose, construction contractors could easily substitute employment of other carpenters for the group's members. Thus, the demand for any one group of carpenters would be highly elastic despite its small share in total cost. In contrast, the demand for all carpenters would be less elastic, as long as the price elasticity of the demand for houses was not high. Put another way, even a relatively small share in total cost cannot "protect" inputs with very good substitutes; their wage elasticities of demand will tend to be elastic.

[5]This example was called to our attention by Mark Killingsworth and is adapted from George J. Stigler, *The Theory of Price*, 4th ed. (New York: Macmillan, 1987), 254. For a formal derivation of the conditions under which this last law holds, see Hicks, *The Theory of Wages*.

Estimates of Own-Wage Labor Demand Elasticities

We started this chapter by pointing out that, to a large extent, the issue of labor demand elasticities is an empirical one. We now turn to the results of studies that estimate own-wage demand elasticities for labor as a generic input (that is, labor undifferentiated by skill level). The estimates we discuss are based on studies that utilize wage, output, and employment data from firms or narrowly defined industries (as opposed to a large aggregation of industries, such as the entire manufacturing sector). Thus, the employment responses being estimated approximate those that would be expected to occur in a firm that had to raise wages to remain competitive in the labor market.

As our analysis has indicated, employers' labor demand responses to a wage change can be broken down into two components: a scale and a substitution effect. These two effects can themselves be expressed as elasticities, and their sum is the own-wage labor demand elasticity. In Table 4.1 we display the results of recent estimates of (*a*) the short-run scale effect, (*b*) the substitution effect, and (*c*) the overall elasticity of demand for labor in the long run.

The scale effect (expressed as an elasticity) is defined as the percentage change in employment associated with a given percentage change in the wage, *holding capital constant*; that is, it is the employment response that occurs without a substitution effect. By definition, the *short-run* labor demand elasticity includes *only* the scale effect, although we noted earlier that the scale effect is likely to be greater in the long run than it is in the short run (owing to greater possibilities for *product market* substitutions in the long run). Therefore, estimates of short-run labor demand elasticities will be synonymous with the short-run scale effect, which may approximate the long-run scale effect if product market substitutions are relatively swift. A study using data from British manufacturing plants estimated that the short-run, own-wage labor demand elasticity is −0.53 (see Table 4.1). The short-run labor demand curve for a typical firm or narrowly defined sector, therefore, would appear to be inelastic.

The substitution effect, when expressed as an elasticity, is the percentage change in employment associated with a given percentage change in the wage rate, *holding output constant*. That is, it is a measure of how employers change their production techniques in response to wage changes, even if output does not change (that is, even if the scale effect is absent). It happens that substitution effects are easier to credibly estimate, so there are many more studies of these effects. One careful summary of 32 studies estimating substitution-effect elasticities placed the average estimated elasticity at −0.45 (which is what is displayed in Table 4.1), with most estimates falling into the range of −0.15 to −0.75.[6]

With the short-run scale elasticity and the substitution elasticity each very close to −0.5, it is not surprising that estimates of the long-run overall elasticity of demand for labor are close to unitary in magnitude. Table 4.1 indicates that a study of plants across several British industries estimated an own-wage elasticity of −0.93, while

[6]Daniel Hamermesh, *Labor Demand* (Princeton: Princeton University Press, 1993), 103.

TABLE 4.1

Components of the Own-Wage Elasticity of Demand for Labor: Empirical Estimates Using Plant-Level Data

	Estimated Elasticity
Short-Run Scale Effect	
British manufacturing firms, 1974–1982	−0.53
Substitution Effect	
32 studies using plant or narrowly defined industry data	Average: −0.45 (Typical range: −0.15 to −0.75)
Overall Labor Demand Elasticity	
British plants, 1984	−0.93
British coal mines, 1950–1980	−1.0 to −1.4

SOURCE: Daniel S. Hamermesh, *Labor Demand* (Princeton: Princeton University Press, 1993), 94–104.

another of British coal mines placed the elasticity of demand for labor in the range of −1.0 to −1.4.[7] Thus, these estimates suggest that if the wages a firm must pay rise by 10 percent, the firm's employment will shrink by close to 10 percent in the long run, other things being equal (that is, unless something else occurs that also affects the demand for labor).

Many more studies estimating the short-run and long-run own-wage elasticities of labor demand must be completed before we can have much confidence in predicting employment responses to changes in labor costs. For now, many policy decisions affecting these costs must be made without definitive predictions concerning their employment effects. The next section illustrates that, fortunately, theory can provide at least some rough guidance about expected magnitudes when precise knowledge is lacking.

Applying the Laws of Derived Demand: Inferential Analysis

Because empirical estimates of demand elasticities that may be required for making decisions are often lacking, it is frequently necessary to try to guess what these elasticities are likely to be. In making these guesses, we can apply the laws of derived demand to predict at least relative magnitudes for various types of labor. Consider first the demand for unionized New York City garment workers. As we shall discuss in chapter 13, because unions are complex organizations, it is not always possible to specify what their goals are. Nevertheless, it is clear that most unions value both wage *and* employment opportunities for their members. This

[7]These estimates are very close to those from an earlier study of coal mines in the United States; see Morris Goldstein and Robert Smith, "The Predicted Impact of the Black Lung Benefits Program on the Coal Industry," in *Evaluating the Labor-Market Effects of Social Programs*, ed. Orley Ashenfelter and James Blum (Princeton: Princeton University Press, 1976).

observation leads to the simple prediction that, other things equal, the more elastic the demand for labor, the smaller will be the wage gain that a union will succeed in winning for its members. The reason for this prediction is that the more elastic the demand curve, the greater will be the percentage employment decline associated with any given percentage increase in wages. As a result, we can expect the following:

1. Unions would win larger wage gains for their members in markets with inelastic labor demand curves;
2. Unions would strive to take actions that reduce the wage elasticity of demand for their members' services; and
3. Unions might first seek to organize workers in markets in which labor demand curves are inelastic (because the potential gains to unionization are higher in these markets).

As we shall see in chapter 13, many of these predictions are borne out by empirical evidence.

Because of foreign competition, the price elasticity of demand for the clothing produced by New York City garment workers is extremely high. Furthermore, employers can easily find other inputs to substitute for these workers—namely, lower-paid nonunion garment workers in the South (this substitution would require moving the plant to the South, a strategy that many manufacturers have followed). These facts lead one to predict that the wage elasticity of demand for New York City unionized garment workers should be very elastic, a prediction that seems to be borne out by union policies in the industry. That is, because the garment workers' union faces a highly elastic demand curve, its wage demands historically have been moderate. However, the union has also aggressively sought to reduce the elasticity of product demand by supporting policies that reduce foreign competition; in addition, it has pushed for higher federal minimum wages in order to reduce employers' incentives to move their plants to the South. (For another illustration of how an elastic *product* demand inhibits union wage increases, see Example 4.1.)

Next, consider the wage elasticity of demand for unionized airplane pilots on commercial scheduled airlines in the United States. Only a small share of the costs of operating large airplanes goes to pay pilots' salaries; such salaries are dwarfed by fuel and capital costs. Furthermore, substitution possibilities are limited; there is little room to substitute unskilled labor for skilled labor (although airlines can contemplate substituting capital for labor by reducing the number of flights they offer while increasing the size of airplanes). In addition, before the deregulation of the airline industry in 1978, many airlines faced no competition on many of their routes or were prohibited from reducing their prices to compete with other airlines that flew the same routes. These factors all suggest that the wage elasticity of demand for airline pilots was quite inelastic. As one might expect, pilots' wages were also quite high because their union could push for large wage increases without fear that these increases would substantially reduce pilots' employment levels. However, after airline deregulation, competition among airline carriers increased substantially, leading to a more elastic labor demand for pilots. As a result, many airlines "requested," and won, reduced wages from their pilots.

EXAMPLE 4.1

Why Are Union Wages So Different in Two Parts of the Trucking Industry?

The trucking industry's "general freight" sector, made up of motor carriers that handle nonspecialized freight requiring no special handling or equipment, is split into two distinct segments. One type of general freight carrier exclusively handles full truckloads, taking them directly from a shipper to a destination. The other type of carrier handles less-than-truckload shipments, which involve multiple shipments on each truck and an intricate coordination of pickups and deliveries. These two segments of the general freight industry have vastly different *elasticities of product demand,* and thus the union that represents truck drivers has a very different ability to raise wages (without suffering unacceptable losses of employment) in each segment.

The full truckload (TL) part of the industry has a product market that is very competitive, because it is relatively easy for firms or individuals to enter the market; one needs only a truck, the proper driver's license, and access to a telephone (to call a freight broker, who matches available drivers with shipments needing delivery). Because this part of the industry has many competing firms, with the threat of even more if prices rise, each firm faces a relatively elastic product demand curve.

Firms specializing in less-than-truckload (LTL) shipments must have a complex system of coordinated routes running between and within cities, and they must therefore be sufficiently large to support their own terminals for storing and transferring shipments from one route to another. The LTL segment of the industry is not easily entered and thus is partially monopolized. From 1980 to 1995—a time period over which the number of TL carriers tripled—virtually the only new entrants into the LTL market were regional subsidiaries of preexisting national carriers! To contrast competition in the two product markets somewhat differently, in 1987 the four largest LTL carriers accounted for 37 percent of total LTL revenues, while the four largest TL carriers accounted for only 11 percent of TL revenues.

The greater extent of competition in the TL part of the industry implies that, at the firm level, *product* demand is more elastic there than in the LTL sector; other things being equal, then, we would expect the *labor* demand curve also to be more elastic in the TL sector. Because unions worry about potential job losses when negotiating with carriers about wages, we would expect to find that union wages are lower in the TL than in the LTL part of the industry. In fact, a 1991 survey revealed that the union mileage rates (drivers are typically compensated on a cents-per-mile basis) were dramatically different in the two sectors:

TL sector
Average union rate: 28.4 cents per mile
Ratio, union to nonunion rate: 1.23

LTL sector
Average union rate: 35.8 cents per mile
Ratio, union to nonunion rate: 1.34

The above data support the theoretical implication that a union's power to raise wages is greater when product (and therefore labor) demand is relatively inelastic. In the less competitive LTL segment of the trucking industry, union drivers' wages are higher, both absolutely and relative to nonunion wages, than they are in the more competitive TL sector.

References: Michael H. Belzer, "Collective Bargaining After Deregulation: Do the Teamsters Still Count?" *Industrial and Labor Relations Review* 48, no. 4 (July 1995): 636–655; and Michael H. Belzer, *Paying the Toll: Economic Deregulation of the Trucking Industry* (Washington, D.C.: Economic Policy Institute, 1994).

Finally, consider the wage elasticity of demand for *domestic* farmworkers. This elasticity will depend heavily on the supply of immigrants, either legal or illegal, who are willing to work as farmworkers at wages less than the wages paid to domestic farmworkers. The successful unionization of farmworkers, coupled with union or government rules that prevent illegal immigrants from accepting such employment, obviously will make the demand curve for domestic farmworkers less elastic. Similarly, government regulations that either limit the quantity of foreign farm products that can be imported into the United States (quotas), place tariffs on such products, or limit foreign producers from *dumping* (selling their farm products in the United States at prices less than they charge in their own countries) will reduce the price elasticity of demand for U.S. farm products (and hence the wage elasticity of demand for domestic farmworkers). This example indicates how government policies on international trade, to be more completely analyzed in the appendix to this chapter, can influence wage elasticities of demand in particular labor markets.

THE CROSS-WAGE ELASTICITY OF DEMAND

Because firms may employ several categories of labor and capital, the demand for any one category can be affected by price changes in the others. For example, if the wages of carpenters rose, more people might build brick homes and the demand for *masons* might increase. On the other hand, an increase in carpenters' wages might decrease the overall level of home building in the economy, which would decrease the demand for *plumbers*. Finally, changes in the price of *capital* could increase or decrease the demand for workers in all three trades.

The direction and magnitude of the above effects can be summarized by examining the elasticities of demand for inputs with respect to the prices of *other* inputs. The *elasticity of demand for input j with respect to the price of input k* is the percentage change in the demand for input *j* induced by a 1 percent change in the price of input *k*. If the two inputs are both categories of labor, these *cross-wage elasticities of demand* are given by

$$\eta_{jk} = \frac{\%\Delta E_j}{\%\Delta W_k} \tag{4.2}$$

and

$$\eta_{kj} = \frac{\%\Delta E_k}{\%\Delta W_j}$$

where, again, the Greek letter η is used to represent the elasticity. If the cross-elasticities are positive (with an increase in the price of one increasing the demand for the other), the two are said to be *gross substitutes*. If these cross-elasticities are negative (and an increase in the price of one reduces the demand for the other), the two are said to be *gross complements* (refer back to Figure 3.3).

It is worth restressing that whether two inputs are gross substitutes or gross complements depends on the relative sizes of the scale and substitution effects. To see this, suppose we assume that adults and teenagers are substitutes in production. A decrease in the teenage wage will thus have opposing effects on adult employment. On the one hand, there is a substitution effect: for a given level of output, employers will now have an incentive to substitute teens for adults in the production process and reduce adult employment. On the other hand, there is a scale effect: a lower teenage wage provides employers with an incentive to increase employment of all inputs, including adults.

If the scale effect proves to be smaller than the substitution effect, adult employment will move in the same direction as teenage wages and the two groups will be gross substitutes. In contrast, if the scale effect is larger than the substitution effect, adult employment and teenage wages will move in opposite directions and the two groups will be gross complements. Knowing that two groups are substitutes in production, then, is not sufficient to tell us whether they are gross substitutes or gross complements.[8]

Because economic theory cannot indicate in advance whether two given inputs will be gross substitutes or gross complements, the major policy questions about cross-wage elasticities of demand relate to the issue of their *sign*; that is, we often want most to know whether a particular cross-elasticity is positive (the inputs are gross substitutes) or negative (they are gross complements). Before turning to a review of actual findings, we analyze underlying forces that determine the signs of cross-elasticities.

Can the Laws of Derived Demand Be Applied to Cross-Elasticities?

The four laws of derived demand developed in the last section *cannot* be applied directly to cross-elasticities; however, *the technological or market considerations that underlie the laws* are still useful in understanding cross-wage elasticities. Stated more fully, the Hicks-Marshall laws of derived demand are based on four technological or market conditions that determine the size of *own-wage* elasticities. Each of the four conditions influences the substitution or the scale effect and, as noted above, the relative strengths of these two effects are also what determine the sign of *cross-elasticities*. The laws that apply to own-wage elasticities cannot be applied directly to cross-elasticities, because with cross-elasticities the substitution effect (if there is one) and the scale effect work in opposite directions. The same (or at least very similar) underlying considerations, however, are basic to an analysis of cross-elasticities.

As we discuss these four considerations in the context of cross-elasticities, it will be helpful to have a hypothetical referent in mind. Let us return, therefore, to the question of what might happen to the demand for adult workers if the wages of teenage workers were to fall. As noted above, the answer depends on the relative strengths of the scale and substitution effects. What determines the strength of each?

The Scale Effect The most immediate effect of a fall in the wages of teenagers would be reduced production costs for those firms that employ them. Competition

[8]As noted in chapter 3, if two groups are complements in production, a decrease in the price of one should lead to increased employment of the other. Complements in production are always gross complements.

in the product market would ensure that lower costs are followed by price reductions, which should stimulate increases in both product demand and the level of output. Increased levels of output will tend to cause increases in employment of all kinds of workers, including adults. This chain of events obviously describes behavior underlying the scale effect, and we now investigate what conditions are likely to make for a strong (or weak) scale effect.

The initial cost (and price) reductions would be greater among those employers for whom teenage wages constituted a higher proportion of total costs. Other things equal, greater price reductions would result in greater increases in both product demand and overall employment. Thus, *the share of total costs devoted to the productive factor whose price is changing* will influence the size of the scale effect. The larger this share is, other things equal, the greater will be the scale effect (and the more likely it is that gross complementarity will exist). This tendency is analogous to the fourth Hicks-Marshall law discussed earlier; the difference is that with cross-elasticities, the factor whose *price* is changing is not the same as the one for which *employment* changes are being analyzed.

The other condition that greatly influences the size of the scale effect is product demand elasticity. In the above case of teenage wage reductions, the greater the increase in product demand when firms reduce their prices, the greater will be the tendency for employment of all workers, including adults, to increase. More generally, *the greater the price elasticity of product demand, other things equal, the greater will be the scale effect (and thus the greater the likelihood of gross complementarity).* The effects of product demand elasticity are thus similar with both own-wage and cross-wage elasticities.

The Substitution Effect After teenage wages fall, firms will also have incentives to alter their production techniques so that teenagers are more heavily used. Whether the greater use of teenagers causes an increase or some loss of adult jobs partially depends on a technological question: Are teenagers and adults substitutes or complements in production? If they are complements in production, the effect on adults of changing productive techniques will reinforce the scale effect and serve to unambiguously increase adult employment (meaning, of course, that adults and teenagers would be gross complements). If they are substitutes in production, however, then changing productive techniques involves using a higher ratio of teenagers to adults, and the question then becomes whether this substitution effect is large or small relative to the scale effect.

A technological condition affecting the size of the substitution effect is a direct carryover from the second Hicks-Marshall law discussed previously: *the substitution effect will be greater when the category of labor whose price has changed is easily substituted for other factors of production.* When analyzing the effects on adult employment of a decline in the teenage wage, it is evident that when teenagers are more easily substituted for adults, the substitution effect (and therefore the chances of gross substitutability between the two categories of labor) will be greater.

Another condition influencing the size of the substitution effect associated with a reduction in the teenage wage relates to the labor supply curve of adults. If the

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adult labor supply curve were upward-sloping and rather steep, then adult wages would tend to fall as teenagers were substituted for adults and the demand curve for adults shifted left. This fall would blunt the substitution effect, because adults would also become cheaper to hire. Conversely, if the adult labor supply curve were relatively flat, adult wages would be less affected by reduced demand and the substitution effect would be less blunted. As with the case of own-wage elasticities (the third Hicks-Marshall law discussed above), *more-elastic factor supply curves thus also lead to a greater substitution effect, other things equal, in the case of cross-wage elasticities.*

Finally, holding other things constant, the share of the teenage wage bill in total costs influences the substitution as well as the scale effect in the example we are analyzing. For example, if teenage labor costs were a very large fraction of total costs, the possibilities for further substitution of teenagers for adults would be rather limited (this can be easily seen by considering an example in which teenagers constituted 100 percent of all production costs). Thus, while a larger share of teenagers in total cost would make for a relatively large scale effect, it also could reflect a situation in which the possibilities of substituting teenagers for adults are smaller than they would otherwise be (smaller, that is, holding other influences constant).

Estimates Relating to Cross-Elasticities

Estimating at least the sign of cross-wage labor demand elasticities is useful for purposes of evaluating public policies, because a policy aimed at one group can have unintended consequences for other groups. For example, as implied above, a policy to subsidize the wages of teenagers could reduce employers' demand for adult workers. Likewise, a policy that reduces the costs of capital investments could create substitution effects that ultimately reduce firms' demand for labor. Thus, it is important to know which categories of labor and capital are substitutes for or complements with each other in the production process. Also, we would like to know whether particular categories of labor exhibit *gross* substitutability or complementarity with each other or with capital.

Most of the cross-wage empirical studies to date have focused on the issue of whether two factors are substitutes or complements in production. These studies estimate the employment response for one category of labor to a wage or price change elsewhere, *holding output constant* (which in effect allows us to focus just on changes in the *mix* of factors used in production). The factors of production paired together for analysis in these studies are numerous and the results are not always clear-cut; nevertheless, the findings taken as a whole offer at least a few generalizations:[9]

1. Labor and energy are clearly substitutes in production, although their degree of substitutability is small.

[9] Hamermesh, *Labor Demand*, 105–127.

2. Labor and materials are probably substitutes in production, with the degree of substitutability again being small.
3. We are not certain whether either skilled or unskilled labor is a substitute for or a complement with capital in the production process. What does appear to be true is that skilled (or well-educated) labor is more likely to be complementary with capital than is unskilled labor—and that if they are both substitutes for capital, the degree of substitutability is smaller for skilled labor.[10]
4. The finding summarized in 3 above suggests that skilled labor is more likely than unskilled labor to be a *gross* complement with capital. This finding is important to our understanding of recent trends in the earnings of skilled and unskilled workers (see chapter 14), because the prices of computers and other high-tech capital goods have fallen dramatically in the past decade or so.
5. The finding in 3 above also implies that if the wages of both skilled and unskilled labor were to rise by the same percentage, the magnitude of any employment loss associated with the substitution effect (as capital is substituted for labor) will be greater for the unskilled. Thus, we expect that, other things equal, *own-wage* labor demand elasticities will be larger in magnitude for unskilled than for skilled workers.
6. The extent of complementarity or substitutability in production between immigrant and native workers, or between new immigrants and older groups of immigrants, is very small. This may help explain the findings, discussed later in chapter 10, that changing flows of immigrants have had relatively minor effects on the wages of native workers.[11]

POLICY APPLICATION: EFFECTS OF MINIMUM WAGE LAWS

History and Description

The Fair Labor Standards Act of 1938 was the first major piece of protective labor legislation adopted at the national level in the United States. Among its provisions were a minimum wage rate, or floor, below which hourly wages could not be reduced, an overtime-pay premium for workers who worked long workweeks, and

[10]Evidence has been offered that the degree of substitutability depends on the age of capital equipment. Specifically, Ann Bartel and Frank Lichtenberg, "Technology: Some Empirical Evidence," *Review of Economics and Statistics* 69 (February 1987): 1–11, present evidence that the relative demand for highly educated workers vis-à-vis less-educated workers declines as the capital stock ages. They attribute this to the comparative advantage that highly educated workers have with respect to learning and implementing new technologies; thus as the capital stock ages, the complementarity of these workers with capital declines.

[11]See George J. Borjas, "The Economics of Immigration," *Journal of Economic Literature* 32, no. 4 (December 1994): 1667–1717, for a review of the literature on this point, especially on pages 1695–1700.

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restrictions on the use of child labor. The minimum wage provisions were designed to guarantee each worker a reasonable wage for his or her work effort and thus to reduce the incidence of poverty.

When initially adopted, the minimum wage was set at \$0.25 an hour and covered roughly 43 percent of all nonsupervisory wage and salary workers—primarily those employed in larger firms involved in interstate commerce (manufacturing, mining, and construction). As Table 4.2 indicates, both the basic minimum wage and coverage under the minimum wage have expanded over time. Indeed, after September 1997, the minimum wage was set at \$5.15 an hour, and over 89 percent of all nonsupervisory workers were covered by its provisions.

It is important to emphasize that the minimum wage rate is specified in *nominal* terms and not in terms *relative* to some other wage or price index. Historically, this specification of the minimum wage has led to a pattern of changes that can be represented by Figure 4.3, where time is plotted on the horizontal axis and the value of the minimum wage relative to average hourly earnings in manufacturing is plotted on the vertical axis. Congress initially specifies the nominal level of the minimum wage (MW_0), which, given the level of average hourly earnings that prevails in the economy (AHE_0), leads to an initial value of the minimum wage relative to average hourly earnings (MW_0/AHE_0). Over time this relative value declines as average hourly earnings increase with inflation or productivity growth. The reduced relative value of the minimum wage creates pressure on Congress to legislate an increase in the nominal minimum wage, and after the passage of time (point t_1 in Figure 4.3) Congress returns the relative value of the minimum wage approximately to its initial level. Over time the process is repeated, and the saw-toothed time profile of relative minimum wage values portrayed in Figure 4.3 emerges. Although it varies from peak to peak, the value of the minimum wage relative to average hourly earnings in manufacturing after each legislated change was typically in the range of 0.45 to 0.50 until the 1990 change, when it dropped to the range of 0.37 to 0.39 (see Table 4.2).

Employment Effects: Theoretical Analysis

Since the minimum wage was first legislated, a concern has been that it will reduce employment, especially among the groups it is intended to benefit. Specifically, in the face of downward-sloping labor demand curves, a policy that compels firms to raise the wages paid to all low-wage workers can be expected to *reduce employment opportunities* for the least-skilled or least-experienced. Thus, while those low-wage workers who remained employed would be helped by an increase in the minimum wage, those who lost jobs could be made poorer. Further, if the percentage loss of employment among low-wage workers is greater than the percentage increase in their wages—that is, if the demand curve for low-wage workers is *elastic*—then the *aggregate* earnings of low-wage workers could be made smaller by an increase in the minimum wage.

Much research effort has been devoted over the years to understanding the employment effects of increases in the minimum wage. In evaluating the findings

TABLE 4.2
Federal Minimum Wage Legislation in the United States, 1938–1997

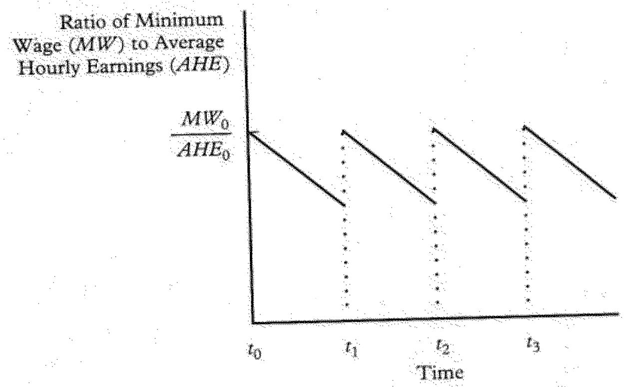
Effective Date of Minimum Wage Change	*Nominal Minimum Wage*	*Percent of Nonsupervisory Employees Covered*[a]	*Minimum Wage Relative to Average Hourly Wage in Manufacturing*[c]	
			Before	*After*
10/24/38	$0.25	43.4	—	0.403
10/24/39	0.30	47.1	0.398	0.478
10/24/45	0.40	55.4	0.295	0.394
1/25/50	0.75	53.4	0.278	0.521
3/1/56	1.00	53.1	0.385	0.512
9/3/61	1.15	62.1	0.431	0.495
9/3/63	1.25	62.1	0.467	0.508
9/3/64	1.25	62.6		
2/1/67	1.40	75.3	0.441	0.494
2/1/68	1.60	72.6	0.465	0.531
2/1/69	1.60	78.2		
2/1/70	1.60	78.5		
2/1/71	1.60	78.4		
5/1/74	2.00	83.7	0.363	0.454
1/1/75	2.10	83.3	0.423	0.445
1/1/76	2.30		0.410	0.449
1/1/78	2.65		0.430	0.480
1/1/79	2.90		0.402	0.440
1/1/80	3.10		0.417	0.445
1/1/81	3.35		0.403	0.435
4/1/90	3.80	88.6[b]	0.329	0.373
4/1/91	4.25		0.342	0.382
10/1/96	4.75	89.5	0.333	0.372
9/1/97	5.15		0.361	0.391

[a]Excludes executive, administrative, and professional personnel (including teachers in elementary and secondary schools) from the base.
[b]As of September 1987.
[c]Ideally, one would like to contrast the value of the minimum wage to average hourly earnings in the economy as a whole. However, prior to 1964 such data were not collected, and hence we express the minimum wage relative to average hourly earnings in manufacturing here to maintain historical comparability. In both 1964 and 1991, average hourly earnings in the private nonfarm sector of the economy were a little over 90 percent of average hourly earnings in manufacturing.

of this research, we must keep in mind that good research has to be guided by good theory. Theory gives us insight into the effects we expect to see from certain causes, thus providing us with a road map that directs our explorations into the real world. With the minimum wage and its effects on employment, we will see that a sophisticated grasp of labor demand theory is necessary in directing us how and where to look for these effects. In particular, theory suggests several issues that must be addressed by any research study of the minimum wage.

FIGURE
Time Pro
Relative

FIGURE 4.3
Time Profile of the Minimum Wage Relative to Average Hourly Earnings



Nominal vs. Real Wages We have already indicated that minimum wage levels in the United States have been set in nominal terms and adjusted by Congress only sporadically. The result is that general price inflation gradually lowers the real minimum wage during the years between congressional action; thus, what appears to be a fixed minimum wage during periods between congressional action turns out to have constantly changing incentives for employment. One will recall that the demand for labor is a downward-sloping function of real wages, so as the real minimum wage falls from the point when it is newly enacted to just before it is raised again, its adverse effects on employment can be expected to decline.

Researchers generally take account of changes in the real minimum wage in one of two ways. First, if looking at employment effects over several years, one can divide the nominal minimum wage by average hourly earnings or some other measure that reflects general price movements.

Second, the federal minimum wage in the United States is uniformly applied to a large country characterized by regional differences in prices. Taking account of regional differences in prices or wages, we find that the real minimum wage in Alaska (where wages and prices are very high) is lower than it is in Mississippi. Recognizing that there are regional differences in the real minimum wage leads to the prediction that employment effects of a uniformly applied minimum wage law generally will be most adverse in regions with the lowest costs of living. (Researchers must also take into account the fact that many states have their own minimum wage laws, some having minimums that exceed the federal minimum.)

Holding Other Things Constant It is important to remember that predictions of job loss associated with higher minimum wages are made *holding other things constant*. In particular, the prediction grows out of what is expected to happen to employment as one moves up and to the left along a *fixed* labor demand curve. If the labor demand curve were to shift at the same time that a new minimum becomes effective, the employment effects of the shift could be confounded with those of the new minimum.

Consider, for example Figure 4.4, where for simplicity we have omitted the labor supply curve and focused on only the demand side of the market. Suppose that D_0 is the demand curve for low-skilled labor in year 0, in which year the real wage is W_0/P_0 and the employment level is E_0. Further assume that in the absence of any change in the minimum wage, the money wage and the price level would both increase by the same percentage over the next year, so that the real wage in year 1 (W_1/P_1) would be the same as that in year 0.

Now suppose that in year 1, two things happen. First, the minimum wage rate is raised to W_2, which is greater than W_1, so that the real wage increases to W_2/P_1. Second, because the economy is expanding, the demand for low-skilled labor shifts out to D_1. The result of these two changes is that employment increases from E_0 to E_1.

Comparisons of observed employment levels at two points of time have led some investigators to conclude that minimum wage increases had no adverse employment effects. However, this simple before/after comparison is *not* the correct one if labor demand has shifted, as in Figure 4.4. Rather, one should ask, "How did the actual employment level in period 1 compare to the level that *would have prevailed* in the absence of the increase in the minimum wage?" Since demand grew between the two periods, this hypothetical employment level would have been E_{1H}. E_{1H} is greater than E_1, the actual level of employment in period 1, so that $E_{1H}-E_1$ represents the loss of jobs caused by the minimum wage. In a growing economy, then, the expected effect of a one-time increase in the minimum wage is to reduce the rate of growth of employment.

Controlling for all the "other things" besides wages that affect labor demand turns out to be the major difficulty in measuring employment changes caused by the minimum wage. A partial way to control for these influences is to examine the effects of a legislated increase in the minimum over a time period so short that changes in consumer preferences or the availability of new products cannot be consequential. Unfortunately, before-and-after studies over a short period of time still must take account of new trends or temporary deviations from old ones, and in any event they capture only the short-run elasticity of demand for labor.

FIGURE 4.4
Minimum Wage Effects: Growing Demand Obscures Job Loss

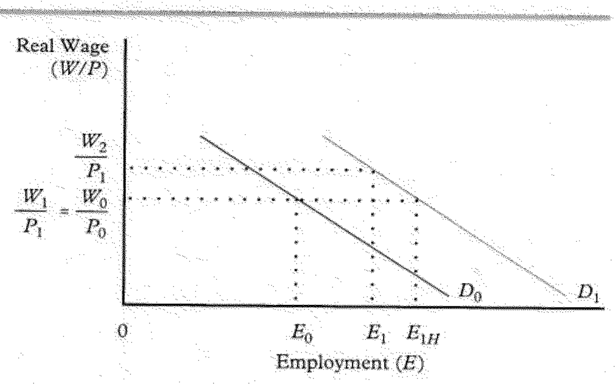
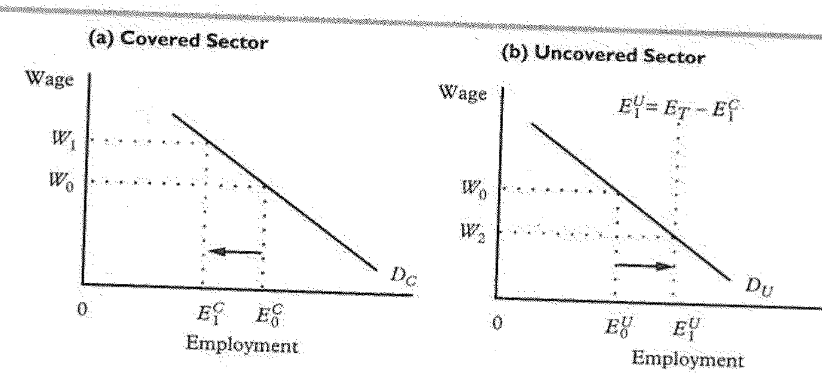


FIGURE 4.5
Minimum Wage Effects: Incomplete Coverage Causes Employment Shifts



Effects of Uncovered Sectors The federal minimum wage law, like many government regulations, has an "uncovered" sector. As can be seen from Table 4.2, coverage has steadily increased over the years, but the law still does not apply to about 10 percent of nonsupervisory workers (the major exemptions are for very small firms in the retail trade and service industries). Also, with millions of employers and limited resources for governmental enforcement, *noncompliance* with the law may be widespread, creating another kind of noncoverage.[12] The existence of uncovered sectors significantly affects how the overall employment of low-wage workers will respond to increases in the minimum wage.

Consider the labor market for unskilled, low-wage workers that is depicted in Figure 4.5. The market has two sectors. In one, employers must pay wages equal to at least the minimum wage of W_1; wages in the uncovered sector are free to vary with market conditions. While the total labor supply to both markets taken as a whole is fixed at E_T (that is, the total labor supply curve is vertical), workers can freely move from one sector to the other seeking better job offers. Free movement between sectors suggests that, in the absence of minimum wage regulations, the wage in each sector will be the same. Referring to Figure 4.5, let us assume that this "pre-minimum" wage is W_0 and that total employment of E_T is broken down into E_0^C in the covered sector plus E_0^U in the uncovered sector.

If a minimum wage of W_1 is imposed on the covered sector, all unskilled workers will prefer to work there. However, the increase in wages in that sector, from W_0 to W_1, reduces demand, and covered-sector employment will fall from E_0^C to E_1^C. Some workers who previously had, or would have found, jobs in the covered sector must now seek work in the uncovered sector. Thus, to the E_0^U workers formerly working in the uncovered sector are added $E_0^C - E_1^C$ other workers seeking jobs there. Thus, all unskilled workers in the market who are not lucky enough to find "covered

[12] Orley Ashenfelter and Robert Smith, "Compliance with the Minimum Wage Law," *Journal of Political Economy* 87 (April 1979): 335–350.

jobs" at W_1 must now look for work in the uncovered sector,[13] and the (vertical) supply curve to that sector becomes E_1^U [$= E_0^U + (E_0^C - E_1^C) = E_T - E_1^C$]. The increased supply of workers to that sector drives down the wage there from W_0 to W_2.

The presence of an uncovered sector thus suggests the possibility that employment among unskilled workers will be rearranged, but not reduced, by an increase in the minimum wage. In the above example, all E_T workers remained employed after the minimum was imposed. Rather than reducing overall employment of the unskilled, then, a partially covering minimum wage law might serve to shift employment out of the covered to the uncovered sector, with the further result that wages in the uncovered sector would be driven down.

The magnitude of any employment shift from the covered to the uncovered sector, of course, depends on the *size* of the latter; the smaller it is, the lower are the chances that job losers from the covered sector will find employment there. Whatever the size of the uncovered sector, however, its very presence means that the *overall* loss of employment is likely to be less than the loss of employment in the *covered sector.*

Intersectoral Shifts in Product Demand It is important to remember that the employment effects of a wage change are the result of scale and substitution effects. Substitution effects stem from changes in the way in which firms choose to produce, while scale effects are rooted in consumer adjustments to changes in product prices. The student will recall that, faced with a given increase (say) in the minimum wage, firms' increases in costs will generally be greater when the share of low-wage labor in total costs is greater; thus, the same increase in the minimum wage can lead to rather different effects on product prices among different parts of the covered sector. Further, if these subsectors compete with each other for customers, it is possible that scale effects of the increased wage will serve to *increase* employment among some firms in the covered sector.

To illustrate how mandated increases in wages can cause intersectoral shifts in product demand, we briefly turn from minimum wages to an analysis of a proposed law (later modified) that would have required coal mine operators to buy insurance for their workers against the risk of dust-related lung disease. The costs of this insurance would have been directly proportional to payroll costs, and the effect would have been to mandate an increase of about 10 percent in the hourly labor costs of miners.[14] Understandably, the government wanted to know what the employment effects of this law might be.

Estimates of the likely employment effects started with the fact that coal mining had two sectors that produced the same product in two very different ways. Under-

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[13]Under some circumstances it may be rational for these unemployed workers to remain unemployed for a while and to search for jobs in the covered sector. We shall explore this possibility—which is discussed by Jacob Mincer in "Unemployment Effects of Minimum Wage Changes," *Journal of Political Economy* 84 (August 1976): S87–S104—in chapter 13. At this point we simply note that if it occurs, unemployment will result.

[14]The details of this example are reported in Morris Goldstein and Robert Smith, "The Predicted [illegible] of the Black Lung Benefits Program on the Coal Industry."

ground mines were very labor-intensive, with roughly half of their total costs associated with labor. Surface mines (also called strip mines) dug coal with huge earth-moving equipment, and labor in this sector constituted only 25 percent of total costs. The mandated insurance, therefore, would have raised costs (and prices) in the underground sector more than in the surface sector, with the result that surface-mined coal would have become *relatively* cheaper. To be sure, increased prices across both sectors would have reduced the *overall* use of coal to some extent, but those who still bought coal would now be more likely to buy it from surface mines than before. The government estimated, in fact, that the demand for surface-mined coal would have increased by about 4 percent if coal mines had been forced to buy this insurance.

In summary, an increased minimum wage may apply equally to all firms in the covered sector, but the employment effects may not be negative in all parts of this sector. An increased minimum will have different effects on product prices in different subsectors, with the result that certain firms may be relatively advantaged even if their total costs go up to some extent! Their costs may rise, but if they rise by less than the costs of their competitors, the new minimum wage actually could serve to stimulate the demand for their products. Measuring the employment effects of minimum wages, then, is best done by looking at an *entire* sector rather than individual firms or narrowly defined subsectors.

Employment Effects: Empirical Estimates

Currently there is no consensus among economists about the effects of minimum wages on employment. After some three hundred studies over the past two decades, what at first seemed like a straightforward way to test the prediction that labor demand curves are negatively sloped has proven to yield results that are frustratingly ambiguous. As a consequence, labor economists are now engaged in a spirited inquiry into whether the problem lies with the use of a theory of labor demand that is too simple or with the research methods used to test that theory.

This textbook is not the place to discuss the intricacies of research design and statistical methodology, but one general feature of the results to date is that reasonable, defensible changes in design or methodology drastically alter estimates of the effects of minimum wages on employment.[15] After our discussion of the theoretical complexities posed by regional differences in living costs, the existence of an uncovered sector, intersectoral shifts within the covered sector, and difficulties in accounting for the "other things" that can affect employment, it is not surprising that research design is critical. What is surprising is that seemingly minor changes in design can have such substantial effects on the results.

One example of the minor changes that cause estimates to vary significantly is found in *time-series* analyses of employment effects among teenagers (a notoriously low-paid group whose wages are likely to be affected by the minimum wage).

[15] Card and Krueger, *Myth and Measurement*, Chapters 6–8.

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These analyses involve estimating how some measure of teenage employment varies over time with the level of the real minimum wage, after controlling for other variables in each year, such as the adult unemployment rate, that could also affect teenage employment. Studies that have applied exactly the same estimating procedures to data from different time periods often obtain dramatically different estimates. For example, an analysis using data from the 1949 to 1994 period estimated that increases in the minimum wage would have no effect on the employment rate (employment divided by population) of 16- and 17-year-olds; applying the same procedures to the 1954 to 1993 period, however, resulted in estimated employment effects that were significantly negative. Another study estimated qualitatively different effects of minimum wages on teenage employment rates over the 1954–1979 and 1954–1986 periods.[16]

A second example of a seemingly minor design change that has caused a major difference in results comes from a comparison of two recent studies of how the employment of teenagers was affected by the 1990 and 1991 increases in the federal minimum wage.[17] Both used data from the same basic source, both analyzed the teenage employment rate in each state for years before and after the increases, and both used each state's yearly *overall* employment rate to partially control for the "other things" that can affect teenage employment. Both also tested, although in different ways, the hypothesis that employment effects will be larger when mandated wage increases are larger.

One study estimated the average relationship, across all states, between the *teenage* and the *overall* employment rates in 1990 and 1991–1992, as compared to the same relationship in the late 1980s. It found that, as expected, the 1990 teenage employment rate was lower, compared to the overall rate, than before; it was lower yet in 1991 and 1992, when the minimum wage was even higher. Further, the declines were greatest for minorities and females, whose wages were most affected by the minimum wage increases. These findings are consistent with the predictions that, other things equal, higher minimum wages will reduce teenage employment opportunities, and that they will reduce them most where mandated wage increases are greatest.

The other study compared 1989 to 1992 changes in states' teenage employment rates with the fraction of teenagers whose wages were affected by the legislated increases in 1990 and 1991, after controlling for changes in each state's overall employment rate over that period. The hypothesis of the study was that employment reductions would be greater in "high-impact" states (states in which the legislation caused the greatest wage increases among teenagers). This study found, contrary to expectations, no evidence that higher minimum wages in 1990 and 1991 reduced teenage employment rates.

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[16]See John Kennan, "The Elusive Effects of Minimum Wages," *Journal of Economic Literature* 33, no. 4 (December 1995): 1950–1965, and Card and Krueger, *Myth and Measurement*, 197.

[17]The first study discussed is Donald Deere, Kevin M. Murphy, and Finis Welch, "Employment and the 1990–1991 Minimum Wage Hike," *American Economic Review* 85, no. 2 (May 1995): 232–237; the second is found in Card and Krueger, *Myth and Measurement*, chapter 4.

The two studies both searched for greater employment effects among the very groups of teenagers whose wages were more likely to be raised by the mandated increases. The former study tested for employment responses associated with two different mandated wage changes on three different demographic groups, while the latter study tested for different responses of the mandated increases in high- and low-impact states. Because both studies employed defensible research designs, one must wonder why such a strong prediction of standard labor demand theory (that the demand curve slopes downward) does not have more robust empirical support.[18]

One is tempted to conclude that even if the employment effects of minimum wages are negative, they are probably relatively small and thus hard to detect. It seems that the impact of the minimum wage on employment and other labor practices was much easier to detect in the earliest days of the Fair Labor Standards Act, as Example 4.2 shows. The estimates of own-wage elasticities cited earlier in this chapter, which derived from very different kinds of studies, suggested that a typical long-run elasticity of demand for labor (as a generic input) was roughly unitary, and we expect the elasticity to be even greater for less-skilled workers and teenagers. After reviewing the estimated labor demand elasticities of teenagers based on changes in the minimum wage, even those who believe that teenage employment declines when the minimum is raised concede that observed labor demand elasticities are far smaller than unitary (an elasticity of -0.1 or -0.3 is typical).[19]

Is it possible that the actual job losses among teenagers are small because uncovered sectors—legal or illegal—absorb those workers displaced from covered employment? If this occurs, we would have found that the 1990–1991 legislated increases, for example, did not increase the overall wages of teenagers. Both studies of the 1990–1991 mandates compared above, however, carefully documented

[18]Empirical support for a downward-sloping labor demand curve from studies of minimum wage laws in other countries has also been mixed, although largely supportive. See, for example, Stephen Machin and Alan Manning, "The Effects of Minimum Wages on Wage Dispersion and Employment: Evidence from U.K. Wage Councils," *Industrial and Labor Relations Review* 47, no. 2 (January 1994): 319–329; John Abowd, Francis Kramarz, Thomas Lemieux, and David Margolis, "Minimum Wages and Youth Employment in France and the United States," working paper no. 6111, National Bureau of Economic Research, Cambridge, Mass., July 1, 1997; John Abowd, Francis Kramarz, and David Margolis, "Minimum Wages and Employment in France and the United States," (mimeo, School of Industrial and Labor Relations, Cornell University, 1998); and Linda A. Bell, "The Impact of Minimum Wages in Mexico and Colombia," *Journal of Labor Economics* 15, no.3, pt. 2 (July 1997): S102–S135. Also, see Organisation for Economic Co-operation and Development, *Employment Outlook June 1998* (Paris: OECD, 1998): 45–47.

[19]For studies on this topic, see Hamermesh, *Labor Demand*, 187; David Neumark and William Wascher, "Employment Effects of Minimum and Subminimum Wages: Panel Data on State Minimum Wage Laws," *Industrial and Labor Relations Review* 46, no. 1 (October 1992): 55–81; Alison J. Wellington, "Effects of the Minimum Wage on the Employment Status of Youths," *Journal of Human Resources* 26, no. 1 (Winter 1991): 27–46; Richard V. Burkhauser, Kenneth A. Couch, and David C. Wittenburg, "A Reassessment of the New Economics of the Minimum Wage Literature Using Monthly Data from the SIPP and CPS" (mimeo, Syracuse University Center for Policy Research, May 1998); and Janet Currie and Bruce C. Fallick, "The Minimum Wage and the Employment of Youth: Evidence from the NLSY," *Journal of Human Resources* 31, no.2 (Spring 1996): 404–428.

EXAMPLE 4.2

The Impact of the First Federal Minimum Wage

When the federal minimum wage first went into effect, on October 24, 1938, it was expected to have a substantial impact on the economy of the South, where wages were much lower than in the rest of the country. An examination of one of the largest manufacturing industries in the South, seamless hosiery, verifies these predictions.

It is readily apparent that the new minimum wage was binding in the seamless hosiery industry. By 1940, nearly one-third of the labor force earned within 2.5 cents per hour of the minimum wage (which was then 32.5 cents per hour). A longitudinal survey of 97 firms shows that employment, which had been rising, reversed course and started to fall, even though overall demand for the product and production levels were rising. Employment fell by 5.5 percent in southern mills but rose by 4.9 percent in northern mills. Even more strikingly, employment fell by 17 percent in mills that had previously paid less than the new minimum wage, while it stayed virtually the same at higher-wage mills.

Before the passage of the minimum wage, there had been a slow movement from the use of hand-transfer to converted-transfer knitting machines. (A converted-transfer machine had an attachment to enable automated production for certain types of work.) The minimum wage seems to have accelerated this trend. In the first two years of the law's existence, there was a 23 percent decrease in the number of hand-transfer machines, a 69 percent increase in converted-transfer machines, and a 10 percent increase in fully automatic machines. In addition, the machines were used more intensively than before. A night shift was added at many mills, and these workers did not receive extra pay for working this undesirable shift. Finally, total imports of seamless hosiery surged by about 27 percent within two years of the minimum wage's enactment.

REFERENCES: Andrew J. Seltzer, "The Effects of the Fair Labor Standards Act of 1938 on the Southern Seamless Hosiery and Lumber Industries," *Journal of Economic History* 57, no. 2 (June 1997), 396–415.

the widespread increase in teenage wages that took place in response to the legislated increases; thus, this possibility is not the most likely explanation. It is somewhat more likely, as we found with our analysis of payroll taxes in chapter 3, that the demand for labor fully adjusts to relatively modest increases in wage costs only with a long lag. If so, before-and-after studies may not be measuring changes in employment over a long enough period to capture the full effects of legislated increases.[20]

[20]A related problem of before-and-after studies (including time-series analyses) of minimum wages is that employers know well in advance the effective date of a new minimum. If some adjust to the new minimum *in advance* of the date selected by the researcher as the "before" period, these adjustments will not be associated by the researcher with the new minimum. For a nice discussion of this problem, see Daniel Hamermesh's comments on Card and Krueger, *Myth and Measurement*, in the volume cited in footnote 2 above.

As noted earlier, however, the rather fragile results of minimum wage studies have also spurred some economists to wonder if the monopsony model of labor demand might have relevance to a wide variety of labor markets.[21] As we saw in chapter 3, one feature of the monopsony model is that it generates ambiguous predictions about how employment might be expected to respond to modest increases in the minimum wage, especially in the short run. The student will remember that the ambiguity concerns only the response of employment to *mandated* wage increases, which flatten the labor supply curve, not to those wage changes generated by shifts in labor supply curves that still leave them upward-sloping. Thus, the monopsony model might help account for the differences in labor demand elasticities based on minimum wage changes and the larger elasticities cited earlier, which were estimated using wage changes generated under different conditions. We will inquire later in this text, especially in chapters 10, 11, and 12, into labor-market characteristics, employment conditions, and issues of supervision and compensation that could create the upward-sloping labor supply curves to firms that are so central to the monopsony model.

Does the Minimum Wage Fight Poverty?

As noted above, the short-run response of low-wage employment to changes in the minimum wage is widely believed to be inelastic; that is, the percentage decline in employment is smaller than the percentage increase in the wage rate. Given an inelastic response of employment, we expect that an increase in the minimum would serve to increase the total earnings going to low-wage workers as a whole. Can it be said, then, that minimum wage laws are effective weapons in the struggle to reduce poverty?

Identifying those who are considered to be living in poverty is done by comparing the income of each family with the poverty line set for families of its particular size; thus, *family* income and family size are the critical variables for defining poverty. Teenagers earning below the minimum, for example, may be benefited if their wages are raised by a legislated increase, but if these teenagers mostly live in nonpoor families, then the increased overall income among teenagers may do very little to reduce poverty.

One study of the 1990–1991 increases in the minimum wage found that, of those who earned between the old and new minimums (that is, between \$3.35 and \$4.24), only 22 percent lived in poor families. Conversely, of those workers in 1990 who lived in poverty, only 26 percent earned between the old and new minimums. All told, assuming no employment effects, only 19 percent of the estimated earnings increases associated with the 1990 and 1991 minimum wage increases went to poor

[21]For a study of monopsony in labor markets in which tips play an important role in compensation, see Walter J. Wessels, "Minimum Wages and Tipped Servers," *Economic Inquiry* 35, no. 2 (April 1997): 334–349.

families.[22] Thus, the minimum wage is a relatively blunt instrument with which to reduce poverty; most of its benefits go to workers in nonpoor families. In chapter 6 we will analyze the strengths and weaknesses of other programs that provide income support to poor families, and in chapter 14 we will raise the question of whether the minimum wage plays a significant role in reducing *earnings* inequality.

APPLYING CONCEPTS OF LABOR DEMAND ELASTICITY TO THE ISSUE OF TECHNOLOGICAL CHANGE

Technological change, which can encompass the introduction of new products and production techniques as well as changes in technology that serve to reduce the cost of capital (for example, increases in the speed of computers), is frequently viewed as a blessing by some and a curse by others. Those who view it positively point to the enormous gains in the standard of living made possible by new technology, while those who see technological change as a threat often stress its adverse consequences for workers. Are the concepts underlying the elasticity of demand for labor useful in making judgments about the effects of technological change?

There are two aspects of technological change that affect the demand for labor. One is product demand. *Shifts* in product demand curves will tend to shift labor demand curves in the same direction, and changes in the *elasticity* of product demand with respect to product price will tend to cause qualitatively similar changes in the own-wage elasticity of labor demand. The invention of new products (word processors, for example) that serve as substitutes for old ones (typewriters) will tend to shift the labor demand curve in the older sector to the left, causing loss of employment in that sector. If greater product substitution possibilities are also created by these new inventions, it is possible that the introduction of new products can increase the *elasticity* of product—and hence, labor—demand. Increasing the own-wage elasticity of labor demand increases the amount of job loss associated with collectively bargained wage increases, for example, and it therefore reduces the power of unions to secure large wage increases in the older sector. While benefiting people as consumers, and while providing jobs in the new sectors, the introduction of new products does necessitate some painful changes in established sectors of the economy as workers, unions, and employers must all adjust to a new environment.

A second aspect of technological change is often associated with automation, or the substitution of capital for labor. For purposes of analyzing its effects on labor demand, this second aspect of technological change should be thought of as reducing

[22]In 1990, the poverty line for a single individual under age 65 was $6,800, while for a family of three it was $10,419 and for a family of four it was $13,359 (see U.S. Bureau of the Census, *Poverty in the United States: 1990*, Series P-60, no. 175, August 1991). The percentages in this paragraph are based on Richard V. Burkhauser, Kenneth A. Couch, and David C. Wittenburg, "'Who Gets What' from Minimum Wage Hikes: A Re-Estimation of Card and Kreuger's Distributional Analysis in *Myth and Measurement: The New Economics of the Minimum Wage*," *Industrial and Labor Relations Review* 49, no. 3 (April 1996): 547–552. Also see Card and Kreuger, *Myth and Measurement*, chapter 9, and David Neumark and William Wascher, "Do Minimum Wages Fight Poverty?" working paper no. 6127, National Bureau of Economic Research, Cambridge, Mass., August 1997.

the cost of capital. In some cases—the mass production of personal computers is one example—a fall in capital prices is what literally occurs. In other cases of technological change—the miniaturization of computer components, for example, which has made possible new production techniques—an invention makes completely new technologies available. When something is unavailable, it can be thought of as having an infinite price (it is not available at any price); therefore, the availability of a new technique is equivalent to observing a decline in its price to some finite number. In either case, with a decline in its cost, capital tends to be substituted for labor in the production process.

Earlier in this chapter, we introduced the concept that the demand for a given category of labor is responsive to changes in the prices of other factors of production; in general, we refer to this responsiveness as a "cross-elasticity" (if the other factor is another category of labor, it is "cross-wage elasticity"). The *sign* of the cross-elasticity of demand for a given category of labor with respect to a fall in the price of capital depends on whether capital and the category of labor are gross substitutes or gross complements. If a particular category of labor is a substitute in production for capital, *and* if the scale effect of the reduced capital price is relatively weak, then capital and the category of labor are gross substitutes and automation reduces demand for workers in this category. For categories of labor that are not close substitutes for the new technology, however, the scale effect may dominate and the two can be gross complements. Thus, the effect of automation on the demand for *particular* categories of labor can be either positive or negative.

Under what conditions are capital and labor most likely to be gross substitutes? Referring back to our earlier discussion, the substitution effect will be stronger to the extent that capital is a substitute for labor in the production process, that it is relatively easy for firms to make the substitution, and that the current share of capital in overall costs is relatively small. The scale effect will be relatively weak if there is an inelastic product demand and if capital constitutes a small share of total cost in the industry experiencing automation.

Clearly, whether capital and a given type of labor are gross substitutes depends on several factors, all of which are highly specific to particular industries and production processes. Perhaps the most that can be said generally is that, as pointed out in a prior section, unskilled labor and capital are more likely to be substitutes in production than are skilled labor and capital, which some studies have identified as complements in production. Because factors of production that are complementary must be gross complements, technological change is more likely to increase the demand for skilled than for unskilled labor.[23]

Before concluding that technological change is a threat to the unskilled, however, three things must be kept in mind. First, even factors that are substitutes in

[23]See David Autor, Lawrence Katz and Alan Krueger, "Computing Inequality: Have Computers Changed the Labor Market?" working paper no. 5956, National Bureau of Economic Research, Cambridge, Mass., March 1997. Alan B. Krueger, "How Computers Have Changed the Wage Structure: Evidence from Microdata, 1984–1989," *Quarterly Journal of Economics* 108 (February 1993): 33–60, concluded that workers who use computers on their jobs earn 10 to 15 percent more than otherwise comparable workers and that the expansion of computer usage in the 1980s accounted for a substantial part of the increased earnings of highly educated workers vis-à-vis less-educated workers that took place during the decade (see chapter 14 for a discussion of this and other earnings changes).

production can be gross complements (if scale effects are large enough). Second, substitution of capital for labor can destroy some unskilled jobs, but accompanying scale effects can create others, sometimes in the same industry.

Finally, although the fraction of all workers who are unskilled laborers has declined over the course of this century, this decline is not in itself convincing evidence of gross substitutability between capital and unskilled labor. The concepts of elasticity and cross-elasticity refer to changes in labor demand caused by changes in wages or capital prices, *holding all else constant*. That is, labor demand elasticities focus on the labor demand curve at a particular point in time. Actual employment outcomes over time are also influenced by labor *supply* behavior of workers. Thus, from simple observations of employment levels over time it is impossible to tell anything about own-wage demand elasticities or about the signs or magnitudes of cross-elasticities of labor demand.

The effects of technological change on *total* employment and on society in general are less ambiguous. Technological change permits society to achieve greater and often more varied consumption possibilities, and it leads to scale effects that both enlarge and change the mix of output. As the productive mix changes, some firms, occupations, and industries decline or are eliminated (see, for example, the data inside the front cover, which show declining employment shares in agriculture and goods-producing industries, where technological changes have reduced the labor required per unit of output). Other sectors of the economy—the services, for example—expand. While these dislocations can create pockets of unemployment as some workers must seek new jobs or acquire new skills, there is no evidence that technological change (over the course of this century, say) has led to permanent problems of unemployment. In fact, real wages have risen rather dramatically from their levels earlier this century, a rise that has been at least partly fueled by technological change.

REVIEW QUESTIONS

1. One organization representing labor argued that interest rates in 1992 were too high and that the government must seek to lower them so that productive, job-creating investments could take place. Do lower interest rates unambiguously increase the demand for labor?
2. A national study concludes that the hourly pay received by part-time workers is always less than the hourly pay received by full-time workers in comparable jobs. It also concludes that part-time workers are disproportionately women, teenagers, the elderly, and the hard-to-employ. Thus, the study suggests that Congress pass a law compelling employers offering part-time jobs to pay the *full-time* wage rate prevailing in their area for the relevant jobs. Analyze as completely as you can the effects such a law would have on both part-time and full-time workers.
3. Many employers provide health insurance for their employees, but others—primarily small employers—do not. Suppose that the government wants to ensure that all employees are provided with health insurance coverage that meets or exceeds some standard. Suppose also that the government wants employers to pay for this coverage and is considering three options:

   Option A: An employer not voluntarily offering its employees acceptable coverage would be required to pay a tax of X cents per hour for each labor hour employed. The funds collected would support government-provided health coverage.

Option B: Same as option A, except that the government-provided coverage would be financed by a tax collected as a fraction of the employer's total revenues.

Option C: Same as option A, except that the government-provided coverage would be financed by a property tax on the buildings, land, and machines owned by the employer.

Compare and contrast the labor market effects of each of the three options.

4. Union A faces a demand curve in which a wage of $4 per hour leads to demand for 20,000 person-hours and a wage of $5 per hour leads to demand for 10,000 person-hours. Union B faces a demand curve in which a wage of $6 per hour leads to demand for 30,000 person-hours, while a wage of $5 per hour leads to demand for 33,000 person-hours.
   a. Which union faces the *more* elastic demand curve?
   b. Which union will be more successful in increasing the total income (wages times person-hours) of its membership?
5. A government intends to pursue policies that will encourage investment in infrastructure (roads, especially), capital goods, and technology. These policies, for example, might involve subsidies of firms' research and development activities, tax credits (tax reductions) for companies that invest in new machinery, or public funding of road building or road repair. Ignoring the issue of how these programs affect tax rates, analyze how each of these policies will affect the labor market.
6. Clerical workers represent a substantial share of the U.S. workforce—over 15 percent in recent years. Concern has been expressed that computerization and office automation will lead to a substantial decline in white-collar employment and increased unemployment of clerical workers. Is this concern well founded?
7. Briefly explain how the following programs would affect the elasticity of demand for labor in the steel industry:
   a. an increased tariff on steel imports;
   b. a law making it illegal to lay off workers for economic reasons;
   c. a "boom" in the machinery industry (which uses steel as an input)—causing production in that industry to rise;
   d. a decision by the owners of steel mills to operate each mill longer than has been the practice in the past;
   e. an increase in the wages paid by employers in the steel industry;
   f. a tax on each ton of steel produced.
8. In 1942 the government promulgated regulations that prohibited the manufacture of many types of garments by workers who did the sewing, stitching, and knitting in their homes. If these prohibitions are repealed, so that clothing items may now be made either by workers in factories or by independent contractors doing work in their homes, what effect will this have on the labor demand curve for *factory workers* in the garment industry?

PROBLEMS

1. Suppose that the demand for dental hygienists is $L_D = 5000 - 20W$, where L = the number of dental hygienists and W = the daily wage. What is the own-wage elasticity of demand for dental hygienists when $W = \$100$ per day? Is the demand curve elastic or inelastic at this point? What is the own-wage elasticity of demand when $W = \$200$ per day? Is the demand curve elastic or inelastic at this point?
2. Professor Pessimist argues before Congress that reducing the size of the military will have grave consequences for the typical American worker. He argues that

if 1 million individuals were released from the military and flooded into the civilian labor market, average wages in the civilian labor market would fall dramatically. Assume that the demand curve for civilian labor does not shift when workers are released from the military. *First*, draw a simple diagram depicting the effect of this influx of workers from the military. *Next*, using your knowledge of (a) the definition of the own-wage elasticity of labor demand, (b) the magnitude of this elasticity for the economy as a whole, and (c) the size of the American labor force in comparison to this flood from the military, graph these events and estimate the magnitude of the reduction in wages for civilian workers as a whole. Do you concur with Professor Pessimist?

3. Suppose that the demand for burger flippers at fast food restaurants in a small city is $L_D = 300 - 20W$, where L = the number of burger flippers and W = the wage in dollars per hour. The equilibrium wage is $4 per hour, but the government puts in place a minimum wage of $5 per hour.
   a. Assuming that none of the firms has any monopsony power, how does the minimum wage affect employment in these fast food restaurants? Draw a graph to show what has happened, and estimate the effects on employment in the fast food sector.
   b. Suppose that in the city above, there is an uncovered sector where $L_S = -100 + 80W$ and $L_D = 300 - 20W$, before the minimum wage is put in place. Suppose that all the workers who lose their jobs as burger flippers due to the introduction of the minimum wage seek work in the uncovered sector. What happens to wages and employment in that sector? Draw a graph to show what happens, and analyze the effects on both wages and employment in the uncovered sector.
4. (Appendix). The production possibilities curve for the United States is linear and allows the country to produce a maximum of 500 million units of clothing or 300 million units of food. The production possibilities curve for France is also linear and allows it to produce a maximum of 250 million units of clothing or 150 million units of food. Which good will the United States export to France?

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The Impact of Pollution on Worker Productivity†

By Joshua Graff Zivin and Matthew Neidell*

As one of the primary factors of production, labor is an essential element in every nation's economy. Investing in human capital is widely viewed as a key to sustaining increases in labor productivity and economic growth. While health is increasingly seen as an important part of human capital, environmental protection, which typically promotes health, has not been viewed through this lens. Indeed, such interventions are typically cast as a tax on producers and consumers, and thus a drag on the labor market and the economy in general. Given the large body of evidence that causally links pollution with poor health outcomes (e.g., Bell et al. 2004; Chay and Greenstone 2003; Currie and Neidell 2005; Dockery et al. 1993; Pope et al. 2002), it seems plausible that efforts to reduce pollution could in fact also be viewed as an investment in human capital, and thus a tool for promoting, rather than retarding economic growth.

The key to this assertion lies in the impacts of pollution on labor market outcomes. While a handful of studies have documented impacts of pollution on labor supply (Carson, Koundouri, and Nauges 2011; Graff Zivin and Neidell forthcoming; Hanna and Oliva 2011; Hausman, Ostro, and Wise 1984; Ostro 1983),[1] their focus on the extensive margin, where behavioral responses are nonmarginal, only captures high-visibility labor market impacts. Pollution is also likely to have productivity impacts on the intensive margin, even in cases where labor supply remains unaffected. Since worker productivity is more difficult to monitor than labor supply, these more subtle impacts may be pervasive throughout the workplace, so that even small individual effects may translate into large welfare losses when aggregated across the economy. There is, however, no systematic evidence to date on the direct impact of pollution on worker productivity.[2] This paper is the first to rigorously assess this environmental productivity effect.

Estimation of this relationship is complicated for two reasons. One, although datasets frequently measure output per worker, these measures do not isolate worker

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[1] Numerous cost-of-illness studies that focus on hospital outcomes such as length of hospital stay also implicitly focus on labor supply impacts.

[2] In a notable case study, Crocker and Horst (1981) examined the impacts of environmental conditions on 17 citrus harvesters. They found a small negative impact on productivity from rather substantial levels of pollution in Southern California in the early 1970s.

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productivity from other inputs (i.e., capital and technology), so that obtaining clean measures of worker productivity is a perennial challenge. Two, exposure to pollution levels is typically endogenous. Since pollution is capitalized into housing prices (Chay and Greenstone 2005), individuals may sort into areas with better air quality depending, in part, on their income, which is a function of their productivity (Banzhaf and Walsh 2008). Furthermore, even if ambient pollution is exogenous, individuals may respond to ambient levels by reducing time spent outside, so that their exposure to pollution is endogenous (Neidell 2009).

In this paper, we use a unique panel dataset on the productivity of agricultural workers to overcome these challenges in analyzing the impact of ozone pollution on productivity. Our data on daily worker productivity is derived from an electronic payroll system used by a large farm in the Central Valley of California that pays it employees through piece rate contracts. A growing body of evidence suggests that piece rates reduce shirking and increase productivity over hourly wages and relative incentive schemes, particularly in agricultural settings (Bandiera, Barankay, and Rasul 2005, 2010; Lazear 2000; Paarsch and Shearar 1999, 2000; Shi 2010). Given the incentives under these contracts, our measures of productivity can be viewed as a reasonable proxy for productive capacity under typical work conditions.

We conduct our analysis at a daily level to exploit the plausibly exogenous daily fluctuations in ambient ozone concentrations. Although aggregate variation in environmental conditions is largely driven by economic activity, daily variation in ozone is likely to be exogenous. Ozone is not directly emitted but forms from complex interactions between nitrogen oxides (NO_x) and volatile organic chemicals (VOCs), both of which are directly emitted, in the presence of heat and sunlight. Thus, ozone levels vary in part because of variations in temperature, but also because of the highly nonlinear relationship with NO_x and VOCs. For example, the ratio of NO_x to VOCs is almost as important as the level of each in affecting ozone levels (Auffhammer and Kellogg 2011), so that small *decreases* in NO_x can even lead to *increases* in ozone concentrations, which has become the leading explanation behind the "ozone weekend effect" (Blanchard and Tanenbaum 2003). Moreover, regional transport of NO_x from distant urban locations, such as Los Angeles and San Francisco, has a tremendous impact on ozone levels in the Central Valley (Sillman 1999). Given the limited local sources of ozone precursors, this suggests that the ozone formation process coupled with emissions from distant urban activities are the driving forces behind the daily variation in environmental conditions observed near this farm.

Furthermore, the labor supply of agricultural workers is highly inelastic in the short run. Workers arrive at the field in crews and return as crews, thus spending the majority of their day outside regardless of environmental conditions. Moreover, since we have measures of both the decision to work and the number of hours worked, we can test whether workers respond to ozone, and in fact we are able to rule out even small changes in avoidance behavior. Thus, focusing on agricultural workers greatly limits the scope for avoidance behavior, further ensuring that exposure to pollution is exogenous in this setting, and that we are detecting productivity impacts on the intensive margin.

Although these workers are paid through piece-rate contracts, worker compensation is subject to minimum wage rules, which can alter the incentive for workers to supply costly effort. Since the minimum wage decouples daily job performance from compensation, workers may have an incentive to shirk. If pollution leads to more workers earning the minimum wage, and this in turn induces shirking, linear regression estimates will be upward biased. On the other hand, the threat of termination may provide a sufficient incentive to provide effort, particularly in our setting where output is easily verified and labor contracts are extremely short-lived, in which case linear regression models should be unbiased.

After merging this worker data with environmental conditions based on readings from air quality and meteorology stations in the California air monitoring network, we first estimate linear models that relate mean ozone concentrations during the typical workday to productivity. We find that ozone levels well below federal air quality standards have a significant impact on productivity: a 10 parts per billion (ppb) decrease in ozone concentrations increases worker productivity by 5.5 percent. To account for potential concerns about shirking, we artificially induce "bottom-coding" on productivity measures for observations where the minimum wage binds, and estimate censored regression models. Under this specification, the actual measures of productivity when the minimum wage binds no longer influence estimates of the impact of ozone on productivity. Thus, if the marginal effects of productivity on this latent variable differ from the marginal effects from our baselir linear model, this would indicate shirking is occurring. Our results, however, remain unchanged, suggesting that the threat of termination provides sufficient incentives for workers to supply effort even when compensation is not directly tied to output.

These impacts are particularly noteworthy as the US Environmental Protection Agency is currently contemplating a reduction in the federal ground-level ozone standard of approximately 10 ppb (Environmental Protection Agency 2010). The environmental productivity effect estimated in this paper offers a novel measure of morbidity impacts that are both more subtle and more pervasive than the standard health impact measures based on hospitalizations and physician visits. Moreover, they have the advantage of already being monetized for use in the regulatory cost-benefit calculations required by Executive Order 12866 (The White House, 1994). In developing countries, where environmental regulations are typically less stringent and agriculture plays a more prominent role in the economy, this environmental productivity effect may have particularly detrimental impacts on national prosperit

The paper is organized as follows. Section I briefly summarizes the relationship between ozone and health, and highlights potentially important confounders. Section II describes the piece-rate and environmental data. Section III provides a conceptual framework that largely serves to guide our econometric model, which is described in Section IV. Section V describes the results, with a conclusion provided in Section VI.

I. Background on Ozone and Health

Ozone affects respiratory morbidity by irritating lung airways, decreasing lung function, and increasing respiratory symptoms (Environmental Protection Agency 2006). Studies have consistently linked higher ozone concentrations with increased health care visits for respiratory diseases (e.g., Neidell 2009), but ozone can also lead to minor insults that may not necessitate the use of formal health care. Fc example, research finds decreases in forced-expiratory volume in animals in

Taiwan (Chan and Wu 2005) and agricultural workers in British Columbia, Canada (Brauer, Blair, and Vedal 1996), even at levels below prevailing air quality standards. Symptoms from ozone exposure can arise in as little as one hour, with effects exacerbated by exercise and with continued duration of exposure (e.g., Gong et al. 1986; Kulle et al. 1985; McDonnell et al. 1983), of which are particularly relevant for our study population given the physical demands of the task and prolonged exposure. How these respiratory changes affect productivity is not well understood, though it is plausible to think that diminished lung functioning would negatively impact productivity for physically demanding work such as that found in agriculture.

Recovery from ozone, once removed from exposure, is also quite rapid. Nearly all lung functioning returns to baseline levels in healthy adults within 24 hours of exposure, although recovery can take longer for hyper-responsive adults with underlying health conditions (Folinsbee and Hazucha 2000; Folinsbee and Horvath 1986).[3] Since ozone levels fall considerably overnight as heat and sunlight decline, we expect lagged ozone to have minimal impacts on the productivity of our healthy worker population. As a result, we focus our analyses primarily on the contemporaneous relationship between ozone and productivity. The impact of lagged ozone concentrations is also explored in order to confirm that our workers are indeed healthy.

As noted in the introduction, ozone formation depends, in part, on ambient temperatures. Human exposure to high temperature can lead to severe negative health effects, including heat cramps, exhaustion, and stroke, as well as more subtle impacts on endurance, fatigue, and cognitive performance (e.g., González-Alonso et al. 1999; Hancock, Ross, and Szalma 2007), all of which may diminish the productivity of workers. The impacts can arise in less than an hour (Hancock, Ross, and Szalma 2007) and are likely nonlinear, as it is mostly temperature extremes outside the "comfort zone" that appreciably affect health (Hancock and Warm 1989). As such, our empirical models will include flexible controls for temperature.

II. Data

Our data comes from a unique arrangement with an international software provider, Orange Enterprises (OE). OE customizes paperless payroll collection for clients, called the Payroll Employee Tracking (PET) Tiger software system. It tracks the progress of employees by collecting real-time data on attendance and harvest levels of individual farm workers in order to facilitate employee and payroll management. The PET Tiger software operates as follows. The software is installed on handheld computers used by field supervisors. At the beginning of the day, supervisors enter the date, starting time, and the crop being harvested. Each employee clocks in by scanning the unique barcode on his or her badge. Each time the employee brings a bushel, bucket, lug, or bin, his or her badge is swiped, recording the unit and time. Data collected in the field is transmitted to a host computer by synchronizing the handheld with the host computer, which facilitates the calculation of worker wages.

We have purchased the rights to daily productivity data from a farm in the Central Valley of California that uses this system. To protect the identity of the farm, we can

[3] Although lung functioning recovers after exposure, long-term damage to lung cells may still occur (Tepper et al. 1989).

only reveal limited information about their operations. The farm, with a total size o roughly 500 acres, produces blueberries and two types of grapes during the warmer months of the year. The farm offers two distinct piece-rate contracts depending on the crop being harvested: time plus pieces (TPP) for the grapes and time plus all pieces (TPAP) for blueberries. Total daily wages (w) from each contract can be described by the following equations:

$$(1)\qquad \text{TPP:}\quad w = 8h + p \cdot (q - minpcs \cdot h) \cdot i(q > minpcs \cdot h)$$

$$\text{TPAP:}\quad w = 8h + p \cdot q \cdot i(q > minpcs \cdot h),$$

where the minimum wage is \$8 per hour, h is hours worked, p is the piece rate, q is daily output, *minpcs* is the minimum number of hourly pieces to reach the piece rate regime, and i is an indicator function equal to 1 if the worker exceeds the minimum daily harvest threshold to qualify for piece-rate wages and 0 otherwise. In both settings, if the worker's average hourly output does not exceed *minpcs*, the worker earns minimum wage. The marginal incentive for a worker whose output places them in the minimum wage portion of the compensation schedule is job security. In TPP, the marginal incentive in the piece rate regime is the piece rate. TPAP slightly differs from TPP in that it pays piece rate for *all* pieces when a worker exceeds the minimum hourly rate (as opposed to paying piece rate only for the pieces above the minimum). Hence, the payoff at *minpcs* is nonlinear and provides a stronger incentive to reach the threshold under this contract. The incentive beyond this kink remains linear as under TPP.

The worker dataset we obtained consists of a longitudinal file that follows workers over time by assigning workers a unique identifier based on the barcode of their employee badge. It includes information on the total number of pieces harvested by each worker,[4] the location of the field, the type of crop, the terms of the piece rate contract,[5] time in and out, and the gender of the worker.[6] Data quality is extremely high, as its primary purpose is to determine worker wages. The analyses in this paper are based on data from the farm for their 2009 and 2010 growing seasons.

Our measures of environmental conditions come from data on air quality and weather from the system of monitoring networks maintained by the California Air Resources Board (2012). These data offer hourly measures of various pollutants and meteorological elements at numerous monitoring sites throughout the state. Th

[4] For one of the three crops, harvests are done in crews of three and individual productivity is measured as the total output of the crew divided by the crew size. While crew work could introduce free-riding incentives, our measure of the environmental productivity effect will only be biased if these incentives change due to pollution. This will only occur if both of the following are true: workers are differentially affected by ozone and the complementarities in team production are very high (e.g., Leontief production). While each member of a crew has a specific task, they typically help each other throughout the day, suggesting that labor is indeed substitutable within th crew. Moreover, Hazucha et al. (2003) find little evidence of heterogeneous health impacts of ozone across healthy men and women. Thus, assigning average productivity measures to individuals within a crew should not bias our estimates.

[5] Piece-rate contracts, and thus minimum daily harvest thresholds, are fixed to the crop for the duration of the season. For simplicity, we label the two types of grapes as two crops given that they have different contracts.

[6] Although we have limited data on the demographic characteristics of our workers, demographics of piece-rate agricultural workers in California obtained from the National Agricultural Workers Survey, an employment-basec random survey of agricultural workers, indicates these workers are poor, uneducated, and speak limited English, with the vast majority migrants from Mexico.

farm is in close proximity to several monitors: three monitors that provide measurements of ozone and other environmental variables are within 20 miles of the farm, with the closest less than 10 miles away.[7] For all environmental variables, we compute an average hourly measure for the typical work day, which starts at 6 am and ends at 3 pm.

We assign environmental conditions to the farm using data from the closest monitoring station to the farm. While studies find that ozone measurements at fixed monitors are often higher than measurement from personal monitors attached to individuals in urban settings (O'Neill et al. 2003), this is less of a concern in the agricultural setting where ratios of personal to fixed monitors have been found to be as high as 0.96 (Brauer and Brook 1995). Furthermore, even when the difference exists, the within-person variation is highly correlated with the within-monitor variation (O'Neill et al. 2003). As a crude test for spatial uniformity of ozone levels, we regress ozone levels from the closest monitor to the farm against the second closest monitor with data available for both years, which is roughly 30 miles away, and obtain an R^2 of 0.85.[8] Thus, despite its simplicity, we expect measurement error using our proposed technique for assigning ozone to the farm to be quite small.

Our data follows roughly 1,600 workers intermittently over 155 days. Table 1 shows summary statistics for worker output and characteristics, environmental variables, and a breakdown of the sample size. There are three main crops harvested by this farm.[9] Under the TPAP contracts, which are used to harvest crop type 1, workers reach the piece-rate regime 24 percent of workdays. For the crops paid under TPP, workers reach the piece-rate regime 57 percent of workdays for crop 2 and 47 percent of workdays for crop 3. Under these contracts, the average hourly wages are \$8.41, \$8.16, and \$8.41 for each of the three crops, respectively. We also see that variation in worker output is equally driven by variation within as well as across workers. Worker tenure with the farm is rather short, averaging 20 days, and both genders are well represented.[10]

In terms of environmental variables, the average ambient ozone level for the day is under 50 ppb, with a standard deviation of 13 ppb and a maximum of 86 ppb. Since this measure of ozone is taken over the average workday from 6 am to 3 pm, it corresponds closely with national ambient air quality standards (NAAQS), which are based on eight-hour ozone measures. Current NAAQS are set at 75 ppb, suggesting that, while ozone levels during work hours can lead to exceedances of air-quality standards, most workdays are not in violation of regulatory standards.[11] Consistent with the area being prone to ozone formation, mean temperature and sunlight (as proxied by solar radiation) are high, and precipitation is low.

[7] To protect the identity of the farm, we cannot reveal the exact distance.

[8] Comparable R^2 for temperature is 0.94 and for particulate matter less than 2.5 μm, another pollutant of much interest, is only 0.27; hence we do not focus on this important pollutant but include it as a covariate.

[9] The timing of the harvest is determined by when each crop is ready to be picked, so workers have little discretion over which crop to harvest on any given day. We explore the potential impact of worker selection into crops in Section VC.

[10] Gender is not reported for 19 percent of the sample.

[11] Violation of NAAQS is based on the daily maximum eight-hour ozone. Since our measure of ozone begins at 6 am, a time when ozone levels are quite low, the daily maximum eight-hour ozone is generally higher than our measure.

Table 1—Summary Statistics

	Observations	Mean	SD	SD within worker	SD between workers
Panel A. Productivity variables (n = 35,461)					
Minimum wage regime					
Time + all pieces, \$0.5/Piece	11,752	2.03	0.57	0.44	0.47
Time + pieces, \$0.3/Piece	3,761	3.07	0.78	0.65	0.70
Time + pieces, \$1/piece	5,918	2.29	0.48	0.31	0.44
Hours worked	21,431	7.64	1.29	0.76	1.20
Piece-rate regime					
Time + all pieces, \$0.5/Piece	3,675	3.42	0.40	0.30	0.32
Time + pieces, \$0.3/Piece	5,115	4.93	0.86	0.70	0.64
Time + pieces, \$1/piece	5,240	3.88	0.82	0.50	0.66
Hours worked	14,030	7.34	1.53	0.96	1.36
Worker characteristics					
Tenure (weeks)	35,461	2.78	2.49		
Percent male	35,461	0.30	0.46		
Percent female	35,461	0.51	0.50		
	Mean	SD	Min	Max	
Panel B. Environmental variables (n = 155)					
Ozone (ppb)	47.77	13.24	10.50	86.00	
Temperature (F)	78.15	8.52	56.30	96.98	
Atmospheric pressure (mb)	1,001.55	6.48	988.86	1,012.59	
Resultant wind speed (mph)	2.74	0.53	1.61	4.60	
Solar radiation (W/m^2)	837.33	174.07	187.00	1,083.33	
Precipitation (mm)	2.40	5.05	0.00	35.48	
Dew point (F)	51.96	5.81	33.14	63.43	
Particulate matter <2.5 ($\mu g/m^3$)	11.69	5.74	1.00	24.44	
Panel C. Sample					
Number of dates	155				
Number of employees	1,664				

Notes: The sample size in panel A refers to worker-days, while the sample size in panel B refers to the number of harvest dates. SD: Standard deviation. Crop 1 is time plus all pieces, with a piece rate of \$0.5/piece and minimum pieces per hour of three. Crop 2 is time plus pieces, with a piece rate of \$0.3/piece and minimum pieces per hour of four. Crop 3 is time plus pieces, with a piece rate of \$1/piece and minimum pieces per hour of three.

For a deeper look at productivity, Figure 1 plots the distribution of average pieces collected per hour by crop and overall, with a line drawn at the rate that correspon with the level of productivity that separates the minimum wage from the piece-rate regime (the regime threshold). To combine productivity across crops, we standardize average hourly productivity by subtracting the minimum number of pieces per hour required to reach the piece-rate regime and dividing by the standard deviation of productivity for each crop, so the value that separates regimes is 0. For the crop paid TPAP, we see evidence of mass displaced just before the regime threshold, which is consistent with the strong incentives associated with just crossing the threshold under this payment scheme. For the two crops paid TPP, the distribution of productivity follows a symmetric normal distribution quite closely, with the exception of some displacement immediately surrounding the regime threshold for crop 2. Since crop 2 is harvested at a rate roughly 50 percent higher than crop 3, as shown in Table 1, it may be easier for workers who are close to the threshold to push themselves just above it by collecting a little more. If shirking occurs when the minimum wage binds, then we would expect part of the distribution to be shifted away from the area just left of the regime threshold and into the left tail. These plots, howeve

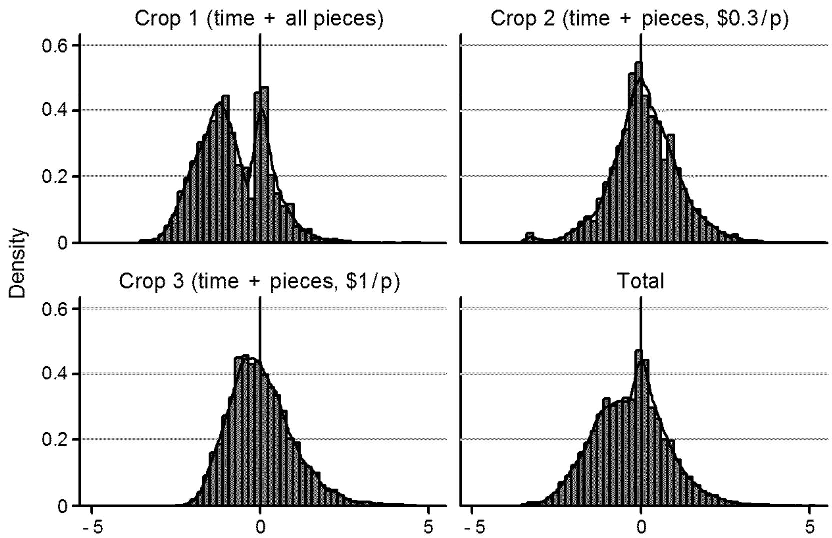

Figure 1. Standardized Average Hourly Pieces Collected by Crop and for All Crops

notes: This figure plots the standardized average hourly pieces for each of the three crops and all crops, along with a nonparametric kernel density estimate. We standardize average hourly productivity by subtracting the minimum number of pieces per hour required to reach the piece-rate regime and dividing by the standard deviation of productivity for each crop. The vertical line reflects the regime threshold for crossing from the minimum wage to the piece-rate regime, which is zero for all crops given the standardization.

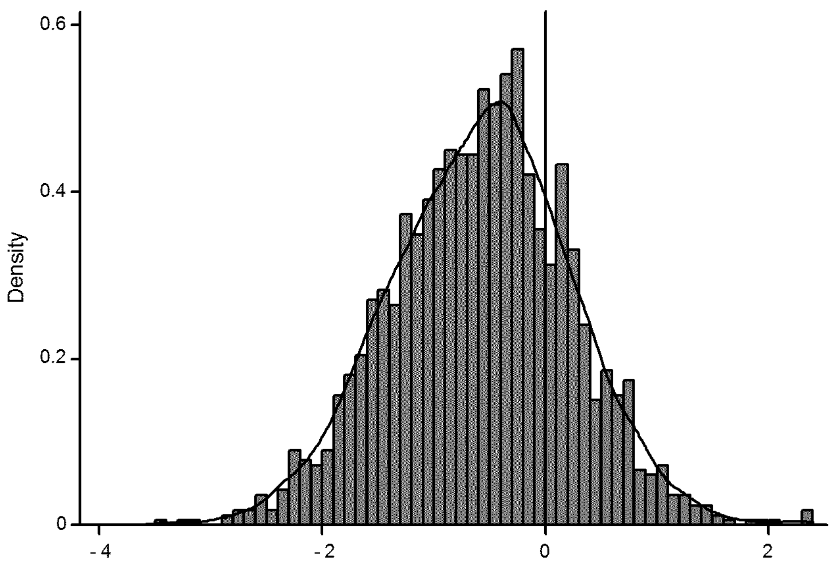

Figure 2. Variation in Productivity by Worker, All Crops

notes: This figure plots the mean of the standardized average hourly pieces for all crops by worker. We standardize average hourly productivity by subtracting the minimum number of pieces per hour required to reach the piece-rate regime and dividing by the standard deviation of productivity for each crop.

do not exhibit such patterns, suggesting that shirking among those receiving a fixed wage is minimal.

The significant variation in pieces collected in Figure 1 is also noteworthy, as this is critical for obtaining precise estimates of the impact of ozone. Figures 2 and 3

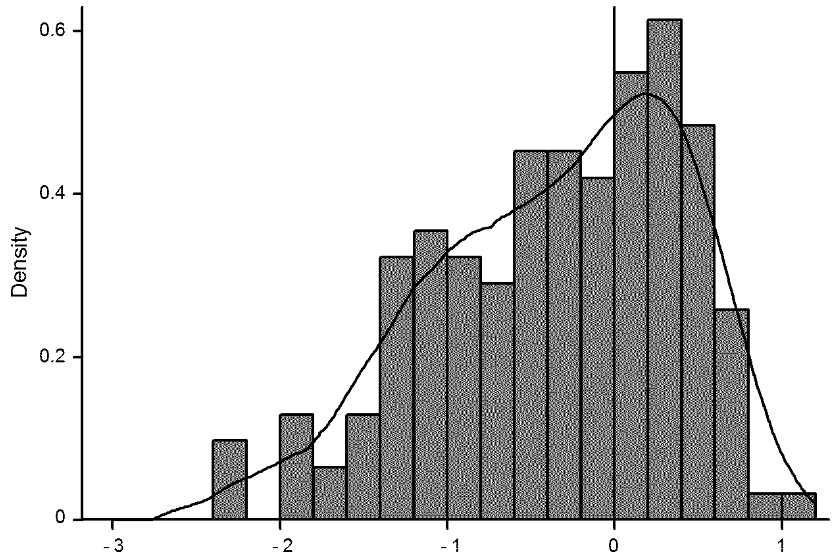

Figure 3. Variation in Productivity by Day, All Crops

Notes: This figure plots the mean of the standardized average hourly pieces for all crops by day. We standardize average hourly productivity by subtracting the minimum number of pieces per hour required to reach the piece rate regime and dividing by the standard deviation of productivity for each crop.

further illustrate this variation both within and across workers. For Figure 2, we collapse the data to the worker level by computing each worker's mean daily productivity over time. For Figure 3, we collapse the data to the daily level by computing the mean output of all workers on each day. This significant variation suggests that both worker ability and environmental conditions appear to be important drivers of worker productivity.

To illustrate the relationship between ozone and temperature, Figure 4 plots the demeaned average hourly ozone and temperature by day separately for the 2009 and 2010 ozone seasons, with an indicator for days on which harvesting occurs for each crop. This Figure reveals considerable variation in both variables over time. Importantly, while ozone and temperature are often correlated—temperature is an input into the production of ozone—there is ample independent variation for conducting our proposed empirical tests.[12] We also control for temperature flexibly to ensure that we are properly accounting for this relationship.

III. Conceptual Framework

In this section, we develop a simple conceptual model to illustrate worker incentives under a piece-rate regime with a minimum wage guarantee. We begin by assuming that the output q for any given worker is a function of effort e and pollution level Ω. Workers are paid piece rate p per unit output, but only if their total daily wage

[12] The R^2 from a regression of ozone on temperature alone is 0.61. When we more flexibly control for temperature and also include additional environmental variables as specified in the econometric model (described below), the R^2 increases to 0.85.

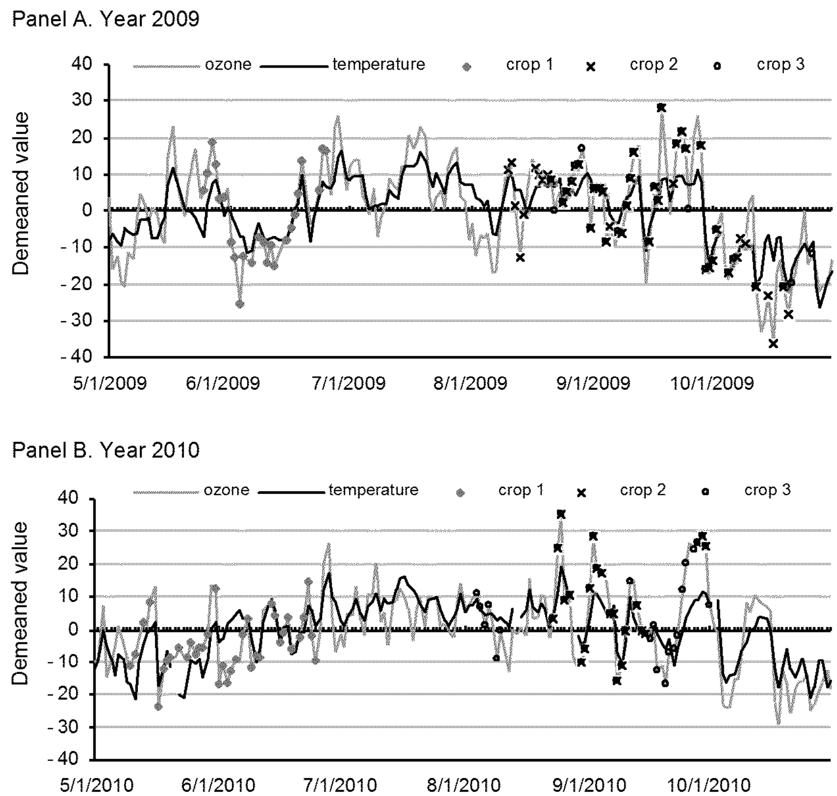

Figure 4. Average Demeaned Daily Ozone and Temperature, and Crop Harvest Days, by Year

note: These figures plot demeaned ozone and temperature levels by day for 2009 and 2010, and indicate the days each of the three crops were harvested.

is at least as large as the daily minimum wage.[13] In anticipation of our empirical model, we let zero denote the threshold level of output at which workers graduat from the minimum wage regime. Since employment contracts are extremely short-lived, we assume that the probability of job retention τ is an increasing function of output levels q when q < 0.[14] Denoting the costs of worker effort as $c(e)$ and the value associated with job retention as k, we can characterize the workers' maximization problem above and below the threshold output level.

For those workers whose output level qualifies them for the piece-rate wage ($q \geq 0$), effort will be chosen in order to maximize the following:

$$\max_{e} \; p \cdot q(e, \Omega) \; - \; c(e). \tag{2}$$

[13] While minimum wage standards are typically fixed at an hourly rate, the fixed-length workday in our setting allows us to translate this into a daily rate.

[14] The assumption of perfect retention for those above the threshold is made for simplicity. As long as the probability of job retention is higher for those workers whose harvest levels exceed the threshold, the basic intuition behind the results that follow remain unchanged.

For those workers whose output level places them under the minimum wage regime ($q < 0$), effort will be chosen to maximize the following:

$$\max_{e} \bar{y} - \tau(q(e,\Omega))\,k - c(e). \tag{3}$$

The first-order conditions for each are

$$p \frac{\partial q}{\partial e} - \frac{\partial c}{\partial e} = 0; \tag{2'}$$

$$-\frac{\partial \tau}{\partial q}\frac{\partial q}{\partial e}k - \frac{\partial c}{\partial e} = 0. \tag{3'}$$

Under the piece-rate regime, workers will supply effort such that the marginal cost of that effort is equal to additional compensation associated with that effort. Fo those workers being paid minimum wage, the incentive to supply effort is driven entirely by concerns about job security.[15] Workers supply effort such that the marginal cost of that effort is equal to the increased probability of job retention associated with that effort times the value of job retention.

The threat of punishment for low levels of output is instrumental in inducing effort under the minimum wage regime. If workers are homogenous and firms set contracts optimally, the gains from job retention due to extra effort will be set equa to the piece-rate wage, i.e., $\frac{\partial \tau}{\partial q}k = p$, such that effort exertion will be identical across both segments of the wage contract. If firms are unable to design optimal contracts, effort will differ across regimes. Of particular concern is the situation i which termination incentives are low-powered, i.e., $\frac{\partial \tau}{\partial q}k < p$. In this case, workers essentially have a limited liability contract, and thus have incentives to shirk unde the minimum wage regime. Moreover, since the productivity impacts of pollution increase the probability of workers falling under the minimum wage portion of the compensation scheme, pollution will also indirectly increase the incentive to shirk, which we must account for in our econometric model.

IV. Econometric Model

The worker maximization problem characterized in the previous section suggests the following econometric model:

$$E[q|\Omega,\mathbf{X}] = P(q \geq 0|\Omega,\mathbf{X}) \times E[q|\Omega,\mathbf{X},q \geq 0] + (1 - P(q \geq 0|\Omega,\mathbf{X})) \times E[q|\Omega,\mathbf{X},q < 0], \tag{4}$$

where P is the probability a worker has output high enough to place them in the piece-rate regime, $1-P$ is the probability a worker's output places them in the

[15] This is conceptually quite similar to the model of efficiency wages and unemployment advanced in Shapiro and Stiglitz (1984), where high wages and the threat of unemployment induce workers to supply costly effort.

minimum wage regime, and **X** are other factors that affect productivity (described in more detail below). We are primarily interested in the direct effect of pollution on productivity (the environmental productivity effect), and use two approaches for estimating this relationship. First, we estimate the following linear model:

$$q = \beta^{ols}\Omega + \theta^{ols}\mathbf{X} + \varepsilon^{ols}, \tag{5}$$

where β^{ols} is the sum of the direct impact and, if it exists, the indirect impact of pollution on productivity via shirking. If the piece-rate contract is set optimally by imposing an appropriate termination threat as described in the previous section, there is no incentive to shirk and β^{ols} will only capture the environmental productivity effect.[16] To the extent that contracts are not set optimally and there is an incentive to shirk in the minimum wage regime, β^{ols} will instead reflect not only the environmental productivity effect, but also the indirect effect due to the interaction of the pollution effect with shirking incentives, and hence provide an upper bound of the estimate of the environmental productivity effect.

To account for potential shirking, as a second approach we estimate equation (4) by artificially "bottom-coding" our data and estimating censored regression models. To do this, we leave all observations in the piece-rate regime as is, but assign a measure of productivity of 0 to all observations in the minimum wage regime.[17] Thus, our estimation strategy can be viewed as a Type I Tobit model of the following form:

$$q^* = \beta^{cen}\Omega + \theta^{cen}\mathbf{X} + \varepsilon^{cen} \tag{6}$$

$$q = q^* \text{ if } q \geq 0$$

$$q = 0 \text{ if } q < 0,$$

where q^* is the latent measure of productivity. Because we are interested in the impact of pollution on actual productivity, which can take on values less than zero, the environmental productivity effect is the marginal effect of pollution on the latent variable q^*, which is simply β^{cen}. Importantly, the actual values of productivity in the minimum wage regime will have no impact on the likelihood function, and hence on β^{cen}. That is, if shirking occurs so that the distribution of productivity in the minimum wage regime is shifted to the left, this shift will no longer influence estimates of β^{cen} because they have been censored. Therefore, even if workers are shirking when paid minimum wage, our estimates of β^{cen} will only capture the environmental productivity effect.

We include data from all crops in one regression by using the standardized measures of productivity described in the data section. We specify ozone in units of 10 ppb since this value is close to prior and recently proposed policy changes for ozone in the United States. Given our standardization of the dependent variable, the

[16] Although environmental conditions may affect workers, they may also have a direct impact on crops. While there is considerable evidence to support the claim that chronic exposure to ozone affects crop yield (see, e.g., Manning, Flagler, and Frenkel 2003), there is no evidence to support an effect from acute exposure.

[17] Because of our standardization of productivity, a value of 0 represents the value when workers switch from the minimum wage to piece rate regime.

coefficients can be interpreted as a standard deviation change in productivity from a 10 ppb change in ozone. To control for other factors that may affect productivity, the vector **X** includes controls for gender, tenure with the farm (a quadratic) temperature, humidity, precipitation, wind speed, air pressure, solar radiation, and fine particulate matter (PM2.5), all measured as the mean over the typical workday. Since ozone is formed in part because of temperature and sunlight, it is essential that we properly control for these variables. To do this, we include a series of temperature indicator variables for every 2.5 degrees Fahrenheit, and also interact these indicators with solar radiation. To control for humidity, we use dew point temperature, a measure of absolute humidity that is not a function of temperature (Barreca 2012), and also include indicator variables for every 2.5 degrees Fahrenheit. We also include a series of day-of-week indicators to capture possible changes in productivity throughout the week, indicator variables for the crop to account for the mean shift in productivity from different contracts, and year-month dummies to control for trends in pollution and productivity within and across growing seasons. All standard errors are two-way clustered on the date because the same environmental conditions are assigned to all workers on a given day and on the worker to account for serial correlation in worker productivity (Cameron, Gelbach, and Miller 2011).

In addition to the aforementioned concerns regarding shirking, several additional primary threats to identification remain. As previously discussed, potential confounding due to weather may bias results, so we control flexibly for temperature and sunlight—two important inputs into the ozone formation process. Furthermore, labor supply decisions may respond to ozone levels. Since we have measures of days and hours worked, we directly explore such responses. Lastly, if there is heterogeneity in the productivity effects of ozone and workers select into crops, this may hinder inference. To assess this, we explore both the heterogeneity of ozone effects and whether ozone or worker characteristics are related to crop assignment.

V. Results

A. *Labor Supply Responses*

We begin by assessing our earlier claim that the labor supply of agricultural workers is insensitive to ozone levels in this setting. We estimate linear regression models for the decision to work and the number of hours worked (conditional on working), both with and without worker fixed effects. Shown in Table 2, the results in the first two columns, which focus on the decision to work, provide no evidence of a labor supply response to ozone.[18] The second two columns also reveal that the number of hours worked is not significantly related to ozone levels. Even at the lower 95 percent confidence interval, a 10 ppb increase in ozone is associated with a 0.28 drop in hours worked, which is a roughly 17-minute decrease in hours worked. The insensitivity of these results to including worker fixed effects strengthens our confidence in these findings. Thus, consistent with our contention that avoidance behavior is not

[18] Marginal effects from logit and probit models for the decision to work are virtually identical to the results from the linear probability model.

Table 2—Regression Results of the Effect of Ozone on Avoidance Behavior

	Extensive margin: probability(work)		Intensive margin: hours worked	
	(1)	(2)	(3)	(4)
Ozone (10 ppb)	0.001	−0.001	0.015	0.026
	[0.026]	[0.027]	[0.149]	[0.154]
Worker fixed effect	N	Y	N	Y
Mean of dep. var.	0.905	0.905	7.52	7.52
Observations	39,223	39,223	35,461	35,461
R^2	0.12	0.17	0.33	0.36

Notes: Standard errors clustered on date and worker in brackets. Hours worked is conditional upon working. All regressions include controls for gender, farm tenure (quadratic), temperature (2.5 degree F indicators), solar radiation, temperature (2.5 degree F indicators) × solar radiation, air pressure, wind speed, dew point (2.5 degree F indicators), precipitation, particulate matter < 2.5 μg, day of week dummies, month × year dummies, and piece rate contract type dummies. All environmental variables are the mean of hourly values from 6 am–3 pm.

Table 3—Main Regression Results of the Effect of Ozone on Productivity

	(1)	(2)	(3)	(4)
Ozone (10 ppb)	−0.143**	−0.174**	−0.164	−0.155
	[0.068]	[0.074]	[0.109]	[0.100]
Model	Linear	Tobit	Median	Censored median
Mean of dep. var.	−0.323	−0.323	−0.323	−0.323
Observations	35,461	35,461	35,461	25,955
(Psuedo) R^2	0.34	0.12	0.22	0.28

Notes: Standard errors clustered on date and worker in brackets. The dependent variable is standardized hourly pieces collected, which is the average hourly productivity minus the minimum number of pieces per hour required to reach the piece rate regime, divided by the standard deviation of productivity for each crop. All regressions include controls for gender, farm tenure (quadratic), temperature (2.5 degree F indicators), solar radiation, temperature (2.5 degree F indicators) × solar radiation, air pressure, wind speed, dew point (2.5 degree F indicators), precipitation, particulate matter ≤ 2.5 μg, day of week dummies, month × year dummies, and piece rate contract type dummies. All environmental variables are the mean of hourly values from 6 am–3 pm. Bootstrapped standard errors for both median regressions were obtained using 250 replications.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

an issue in this setting, farm workers do not appear to adjust their work schedules in response to ozone levels.

B. *Main Productivity Results*

In Table 3, we present our main results. Column 1 presents results from our linear regression model. The estimated coefficient suggests that a 10 ppb increase in ozone leads to a statistically significant decrease in productivity of 0.143 of a standard deviation.[19] Based on the distribution of ozone and productivity in our sample, this estimate implies that a 10 ppb decrease in ozone increases worker

[19] Although we control for other local pollutants that might affect productivity, such as PM2.5, we do not contro
for NO_x because it is a precursor to ozone formation. The transport of ozone, however, suggests that most of the

productivity by 5.5 percent. If wage contracts are set optimally, this is an unbiased estimate of the effect of ozone pollution. If contracts are not set optimally and workers shirk when the minimum wage binds, then this estimate will overstate the impact of ozone. In column 2 we show results from a Type I Tobit model, where we artificially censor observations when the minimum wage binds, and find a slightly larger estimate of 0.174 standard deviation effect from a 10 ppb change in ozone, with the difference not statistically different from those found under the linear model.[20]

Since this Tobit model assumes normality and homoskedasticity, we assess the sensitivity of our results to these assumptions by estimating a censored median regression model, also displaying results from an uncensored median regression model as a reference point.[21] Shown in column 3, the median regression estimate of 0.164 is quite comparable to the linear regression estimate, which is not surprising given the distribution of productivity shown in Figure 1. The censored median regression estimate of 0.155, shown in column 4, is also quite similar to the estimates from the parametric censored models, lending support to the parametric assumptions of the Tobit model. The comparability of the four estimates in this table suggests that shirking due to the minimum wage is relatively minimal in this setting. Thus, the basic linear regression specification appears to yield unbiased estimates of the pollution productivity effect.[22]

In Table 4, we explore the sensitivity of the linear estimates to various additional assumptions. Column 1 repeats the baseline results. In column 2 we include worker fixed effects. Although this increases the explanatory power of our regressions considerably, the estimates for ozone fall somewhat to 0.101, though this change is not statistically significant. Thus, consistent with the notion that workers are not selecting into employment on any given day based on ozone concentrations, cross-sectional and fixed effects estimates are quite similar.

Figure 1 provided some evidence that worker effort changes near the regime threshold, particularly for crop 1 where contracts are TPAP. If higher ozone levels reduce productivity and hence make it more likely for workers to fall into the minimum wage regime, this offsetting increase in effort may bias our results downward. In the next two columns of Table 4, we address this by excluding observations that are close to the regime threshold, varying our definition of "close." Consistent with expectations, our results are slightly larger as we exclude more observations, but these differences are minimal.

While our data agreement entitles us to productivity data aggregated to the daily level, we have time-stamped measures for crop 1, thus allowing us to explore how the impacts of ozone vary throughout the day. There are two notable limitations in

NO_x that contributes to the production of ozone is emitted in urban centers far from the farm. Consistent with this, if we add a control for local NO_x, the coefficient on ozone changes minimally.

[20] Consistent with these results, if we specify the dependent variable as the probability the worker reaches the piece-rate regime, we find that ozone reduces this probability by 5.9 percentage points and is statistically significant at the 10 percent level.

[21] We estimate a censored median model using the three-step procedure developed by Chernozhukov and Hong (2002).

[22] Consistent with the notion that shirking may be minimized through the threat of termination, we find that workers in the lower deciles of the productivity distribution are much more likely to separate from the farm than those in the upper deciles (unreported results available upon request from the authors).

Table 4—Sensitivity of Regression Results of the Effect of Ozone on Productivity

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Ozone (10 ppb)	−0.143** [0.068]	−0.101* [0.059]	−0.148** [0.075]	−0.160** [0.080]	−0.197*** [0.0683]	−0.197*** [0.0686]	−0.248*** [0.0788]	−0.229*** [0.0842]
1 lag ozone (10 ppb)						0.004 [0.045]		−0.066 [0.056]
2 lag ozone (10 ppb)								0.114** [0.0493]
Sum of coefficients						−0.193 [0.076]**		−0.182 [0.100]*
Model	Baseline	Worker fixed effect	Exclude obs. 0.1 SD of regime threshold	Exclude obs. 0.2 SD of regime threshold	Exclude Monday	Exclude Monday	Exclude Monday and Tuesday	Exclude Monday and Tuesday
Mean of dep. var.	−0.323	−0.323	−0.360	−0.389	−0.235	−0.235	−0.183	−0.183
Observations	35,461	35,461	31,706	29,376	25,456	25,456	17,498	17,498
R^2	0.34	0.59	0.36	0.38	0.36	0.36	0.35	0.36

notes: Standard errors clustered on date and worker in brackets. The dependent variable is standardized hourly pieces collected, which is the average hourly productivity minus the minimum number of pieces per hour required to reach the piece rate regime, divided by the standard deviation of productivity for each crop. All regressions are based on linear models that include controls for gender, farm tenure (quadratic), temperature (2.5 degree F indicators), solar radiation, temperature (2.5 degree F indicators) × solar radiation, air pressure, wind speed, dew point (2.5 degree F indicators), precipitation, particulate matter < 2.5 μg, day of week dummies, month × year dummies, and piece rate contract type dummies. All environmental variables are the mean of hourly values from 6 am–3 pm.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

this intraday analysis: (i) while pieces can be delivered at any time, environmental variables are measured by clock hour; and (ii) workers sometimes deliver several pieces at once. As a result, we construct hourly productivity measures using linear interpolation. We then use this linearly interpolated hourly data to examine intrad impacts by interacting ozone with the hour of the day, also controlling for hour of th day to account for changes in fatigue as the day progresses. Although the estimate for each hour is not statistically significant at conventional levels, which is not surprising given the measurement error induced by interpolation, the estimates suggest a pattern whereby ozone begins to impact productivity by 10 am and remains fairly steady from that point onward (results available upon request).

To address potential concerns about the cumulative effect of ozone exposure, we also present results that include one- and two-day lags of ozone. Since ozone levels may only reflect exposure on days when workers actually work, we limit our focus to days when workers have worked the previous day by excluding from our analysis the first one or two days of the workweek depending on how many lags we include in our specification. Shown in column 5 of Table 4 are results without any lags but excluding Monday, which are slightly higher than the baseline results. Including one lag of ozone, shown in column 6, we find that the coefficient on contemporaneous ozone remains the same, and lagged ozone is negative but statistically insignificant. The results in column 7 show that excluding the first two workdays continues to increase the contemporaneous coefficient on ozone. Including two lags of ozone, column 8 shows that the coefficient on contemporaneous ozone remains statistically

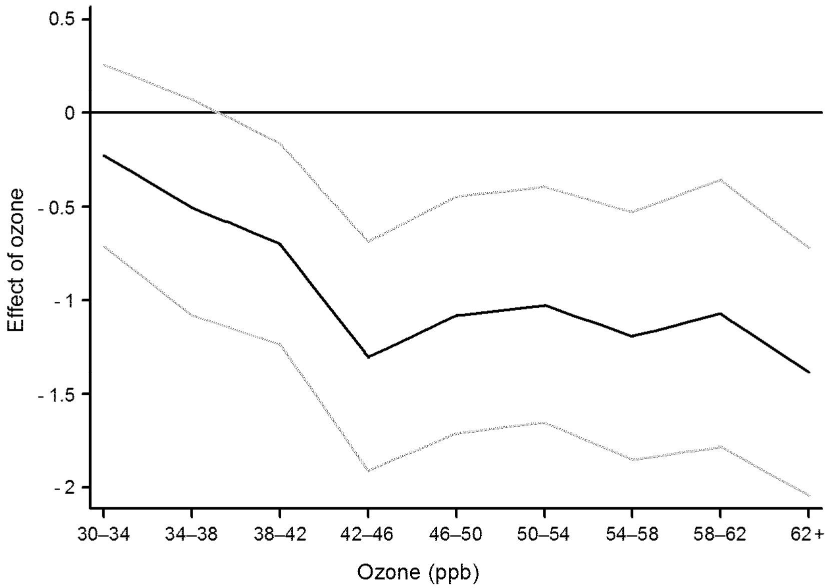

Figure 5. Regression Results of the Effect of Ozone on Productivity Using More Flexible Controls for Ozone

Notes: This figure plots the coefficients for the ozone indicator variables (< 30 ppb reference category), with the 95 percent confidence interval based on standard errors clustered on date and worker in gray. The dependent variable is standardized hourly pieces collected, which is the average hourly productivity minus the minimum number of pieces per hour required to reach the piece rate regime, divided by the standard deviation of productivity for each crop. The regression includes controls for gender, farm tenure (quadratic), temperature (2.5 degree F indicators), solar radiation, temperature (2.5 degree F indicators), solar radiation, air pressure, wind speed, dew point (2.5 degree F indicators), precipitation, particulate matter < 2.5 μm, day of week dummies, month-year dummies, and piece rate contract type dummies. All environmental variables are the mean of hourly values from 6 am–3 pm.

significant and again unchanged, while one lag of ozone is statistically insignificant and the second lag is significant but positive, with colinearity of ozone across days as one possible explanation for the seemingly perverse sign. Most notably, the sum of the ozone coefficients is quite close to the contemporaneous effect regardless of the lags included. Together, these estimates suggest that the predominant effect of ozone is from same-day exposure, with an overnight respite from ozone sufficient for lung functioning to return to baseline levels. Moreover, this rapid recovery implies that the environmental productivity effects measured in this paper are predominantly impacting a healthy population.[23]

Throughout our analysis, we have assumed ozone has a linear effect on productivity. In Figure 5, we present estimates that allow for a nonlinear effect by including indicator variables for every 4 ppb of ozone, omitting < 30 ppb as the reference category. As shown, the figure illustrates a relatively linear and steady increase in the productivity impacts of ozone over the entire range of ozone. Perhaps more importantly, the impacts appear to become statistically significant at 42–46 ppb, a

[23] Recall from Section II that chamber studies suggest a rapid recovery from ozone exposure for healthy individuals. As further evidence consistent with these workers being generally healthy, we find that lagged ozone levels are not significantly related to the decision to work.

Table 5—Heterogeneity of Regression Results of the Effect of Ozone on Productivity

	(1)	(2)	(3)	(4)	(5)
Ozone (10 ppb)	−0.143**	−0.149**	−0.169**	−0.135*	−0.006
	[0.068]	[0.075]	[0.069]	[0.076]	[0.041]
Ozone (10 ppb) × tenure		−0.007			
		[0.015]			
Ozone (10 ppb) × tenure^2		0.002			
		[0.001]			
Ozone (10 ppb) × female			0.040**		
			[0.017]		
Ozone (10 ppb) × unknown			0.029		
			[0.025]		
Ozone (10 ppb) × crop1				−0.216***	
				[0.071]	
Ozone (10 ppb) × crop2				0.149**	
				[0.060]	
Tenure	0.038*	0.083	0.039*	0.054**	0.000
	[0.023]	[0.077]	[0.023]	[0.022]	[0.015]
Tenure^2	−0.002	−0.013*	−0.002	−0.003*	0.002
	[0.002]	[0.007]	[0.002]	[0.002]	[0.001]
Female	−0.094***	−0.092***	−0.284***	−0.093***	0.257***
	[0.035]	[0.035]	[0.083]	[0.035]	[0.039]
Unknown	0.069	0.068	−0.07	0.062	0.093*
	[0.050]	[0.050]	[0.125]	[0.049]	[0.053]
Model	Baseline	Tenure interaction	Gender interaction	Crop interaction	y = pr(crop 2)
Mean of dep. var.	−0.323	−0.323	−0.323	−0.323	0.443
Observations	35,461	35,461	35,461	35,461	20,034
R^2	0.344	0.346	0.345	0.356	0.201

Notes: Standard errors clustered on date and worker in brackets. The dependent variable in columns 1–4 is standardized hourly pieces collected, which is the average hourly productivity minus the minimum number of pieces per hour required to reach the piece rate regime, divided by the standard deviation of productivity for each crop. The dependent variable in column 5 is whether the worker harvested crop 2, and the sample is restricted to days when only crop 2 or 3 are harvested. In addition to covariates shown, all regressions are based on linear models that include controls for temperature (2.5 degree F indicators), solar radiation, temperature (2.5 degree F indicators) × solar radiation, air pressure, wind speed, dew point (2.5 degree F indicators), precipitation, particulate matter < 2.5 μ_g, day of week dummies, month × year dummies, and piece rate contract type dummies. All environmental variables are the mean of hourly values from 6am–3pm. "Unknown" indicates that gender was not reported in our data.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

concentration well below current air quality standards of 75 ppb or even proposed reforms of 60 ppb.

C. *Heterogeneity of Productivity Results*

To assess whether individuals are differentially affected by ozone, we explore potential heterogeneity by interacting ozone with the limited worker characteristics in our dataset (tenure with the farm and gender) and with the crop, shown in Table 5.[24] While

[24] We also estimated quantile regression models for each decile of worker productivity, and found that ozone has a similar effect on worker productivity throughout the entire productivity distribution (results available upon request)

workers with more experience may be more resilient to ozone by being better able to pace themselves throughout the day, column 2 finds no such evidence. Interacting ozone with a quadratic in tenure is statistically insignificant and the level effect of ozone is largely unchanged. Shown in column 3, we find that ozone has a smaller impact on productivity for women.[25] While the magnitude of the difference between the effect for men and women is quite small, this result is contrary to laboratory studies that generally find no differential impact on lung functioning by gender (Hazucha, Folinsbee, and Bromberg 2003). Column 4 interacts ozone with crop dummy variables and reveals considerable heterogeneity in the productivity effects of ozone. The effect for crop 1 is significantly larger than crop 3 (the reference category), while the effect for crop 2 is significantly smaller. Since crops 2 and 3 are both paid time plus pieces, these differences are not driven by the different contract types.

To understand this source of heterogeneity, we first explore whether worker assignment to crop may explain these patterns. To assess this, we run a regression to predict working on crop 2, limiting our sample to days when only crop 2 or 3 is harvested (since crop 1 is harvested in a different time period). As shown in column 5, gender is related to crop assignment: females are more likely to select into crop 2. Given that females are less affected by ozone, this suggests that gender selection into crops may explain some of this heterogeneity. Based on estimates from columns 3–5, however, gender selection can only explain 7 percent of the crop heterogeneity, suggesting that other factors must explain the differential effects by crop.[26] Importantly, ozone is not related to crop assignment, confirming that our estimates represent a valid estimate of the average treatment effect across the crops.

One explanation for this heterogeneity may be the differing physical demands placed on workers across crops. While crops 2 and 3 (grapes) are trellised such that harvestable fruit is waist to shoulder height, crop 1 (blueberries) grows closer to the ground, which requires considerable bending for workers and thus requires more energy to harvest. Within grapes, the crop 2 varietal is a delicate one that requires a slower and more careful harvest to avoid fruit damage, thus placing less physical demands on workers. Therefore, our findings that crop 1, which places the greatest physical demands on workers, is most affected by ozone and crop 2, which places the least physical demands, is least affected is consistent with laboratory studies (discussed in Section II) that find lung functioning impairment due to ozone is exacerbated by exercise.

VI. Conclusion

In this paper, we merge a unique dataset on individual-level daily harvest rates for agricultural workers with data on environmental conditions to assess the impact of ozone pollution on worker productivity. We find that a 10 ppb change in average ozone exposure results in a significant and robust 5.5 percent change in agricultural worker productivity. Importantly, this environmental productivity effect suggests

[25] Despite the smaller impact of ozone for females, the coefficient on gender reveals that female productivity is considerably lower than male productivity on average. As discussed in Table 1, gender is not reported for roughly 19 percent of the sample.

[26] We obtain this estimate of 7 percent by multiplying the differential effect of ozone by gender (0.04) by the selection into crop 2 (0.257), and dividing it by the amount of heterogeneity (0.149).

that common characterizations of environmental protection as purely a tax on producers and consumers to be weighed against the consumption benefits associated with improved environmental quality may be misguided. Environmental protection can also be viewed as an investment in human capital, and its contribution to firm productivity and economic growth should be incorporated in the calculus of policymakers.

Our results also speak to the ongoing debates on ozone policy. Ozone pollution continues to be a pervasive environmental issue throughout much of the world. Debates over the optimal level of ozone have ensued for many years, and current efforts to strengthen these standards remain contentious. Defining regulatory standards depends, in part, on the benefits associated with avoided exposure, which has traditionally been estimated through a focus on high-visibility health effects such as hospitalizations. The labor productivity impacts measured in this paper help make these benefits calculations more complete. Our results indicate that ozone, even at levels below current air-quality standards in most of the world, has significant negative impacts on worker productivity, suggesting that the strengthening of regulation on ozone pollution would yield additional benefits.

These impacts of ozone on agricultural workers are also important in their own right. A back-of-the envelope calculation that applies the environmental productivity effect estimated in the Central Valley of California to the whole of the United States suggests that the 10 ppb reduction in the ozone standard currently being considered by EPA would translate into an annual cost savings of approximately $700 million in labor expenditure.[27] In the developing world, where national incomes depend more heavily on agriculture, these productivity effects are likely to have a much larger impact on the economy and the well-being of households. Nearly 1.1 billion individuals—35 percent of the active labor force—work in the agricultural sector worldwide (International Labour Organization 2011). The impacts of ozone may be especially large in countries like India, China, and Mexico, where rapid industrial growth and automobile penetration contribute precursor chemicals that contribute to substantially higher levels of ozone pollution.

While the impacts of ozone on agricultural productivity are large, the generalizability of these findings to other pollutants and industries is unclear. Agricultural workers face considerably higher levels of exposure to pollution than individuals who work indoors. That said, roughly 11.8 percent of the US labor force works in an industry with regular exposure to outdoor conditions, and this figure is much higher for middle- and lower-income countries (Graff Zivin and Neidell forthcoming). Moreover, many forms of outdoor pollution diminish indoor air quality as well. For example, indoor penetration of fine particulate matter ranges from 38–94 percent for typical residential homes in the United States (Abt et al. 2000). Examining the generalizability of the environmental productivity effect estimated in this paper to other pollutants and industries represents a fruitful area for future research.

[27] Total labor expenditure in US agriculture was approximately $26.5 billion in 2007 (United States Department of Agriculture 2009). Ozone season in California runs from April through October. Using the conservative assumption that the seasonal distribution of agricultural labor expenditure is flat (it is likely lower in winter) yields a total annual expenditure of $13.25 billion that is exposed to ozone productivity risk. The calculation assumes that the new standard shifts the entire distribution of ozone down by 10ppb and not just values that exceed air quality standards.

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The Impacts of Environmental Regulations on Industrial Activity: Evidence from the 1970 and 1977 Clean Air Act Amendments and the Census of Manufactures

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This paper estimates the impacts of the Clean Air Act's division of counties into pollutant-specific nonattainment and attainment categories on measures of industrial activity obtained from 1.75 million plant observations from the Census of Manufactures. Emitters of the controlled pollutants in nonattainment counties were subject to greater regulatory oversight than emitters in attainment counties. The preferred statistical model for plant-level growth includes plant fixed effects, industry by period fixed effects, and county by period fixed effects. The estimates from this model suggest that in the first 15 years

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in which the Clean Air Act was in force (1972–87), nonattainment counties (relative to attainment ones) lost approximately 590,000 jobs, $37 billion in capital stock, and $75 billion (1987 dollars) of output in pollution-intensive industries. These findings are robust across many specifications, and the effects are apparent in many polluting industries.

I. Introduction

Efforts to regulate pollution are among the federal government's most controversial interventions into the marketplace. On the one hand, the Pollution Abatement Costs and Expenditures Survey reports that manufacturing plants spend almost $30 billion a year to comply with environmental regulations (U.S. Bureau of the Census 1993). Manufacturers contend that these expenditures place them at a competitive disadvantage in the global economy and that this leads to the loss of tens of thousands of U.S. jobs. On the other hand, previous empirical research fails to consistently document a negative association between environmental regulations and industrial activity (Bartik 1985; McConnell and Schwab 1990; Gray and Shadbegian 1995; Jaffe et al. 1995; Henderson 1996; Levinson 1996; Becker and Henderson 2000, 2001). In fact, some research suggests that environmental regulations do not harm regulated firms or their workers and may even benefit them (Porter and van der Linde 1995; Berman and Bui 1998, 2001). To set rational policy, it is crucial to understand whether these regulations restrict economic progress.[1] This paper presents new evidence about the relationship between environmental regulations and industrial activity by focusing on the Clean Air Act's impact on polluting manufacturers.

The Clean Air Act, originally passed in 1963 and amended in 1970, 1977, and 1990, is one of the most significant federal interventions into the market in the postwar period. Following the passage of the 1970 amendments, the Environmental Protection Agency (EPA) established separate national ambient air quality standards—a minimum level of air quality that all counties are required to meet—for four criteria pollutants: carbon monoxide (CO), tropospheric ozone (O_3), sulfur dioxide (SO_2), and total suspended particulates (TSPs). As a part of this legislation, every U.S. county receives separate nonattainment or attainment designations for each of the four pollutants annually. The nonattainment designation is reserved for counties whose air contains concentrations of a pollutant that exceed the relevant federal standard. Emitters of the regulated pollutant in nonattainment counties are sub-

[1] See Chay and Greenstone (2000, 2002*a*) for estimates of the benefits associated with the Clean Air Act Amendments.

ject to stricter regulatory oversight than emitters in attainment counties. Nonpolluters are free from regulation in both categories of counties.

This paper brings together a variety of comprehensive data files to empirically determine the effects of these federally mandated county-level regulations on the activity of polluting manufacturers in the 1967–87 period. I compiled annual data on the four pollutant-specific, nonattainment/attainment designations for each of the 3,070 U.S. counties from the *Code of Federal Regulations* and EPA pollution monitors. The structure of these longitudinal regulation data allows for the identification of cross-sectional variation in these regulations, as well as changes in counties' pollutant-specific regulatory status over time. Despite the centrality of these county-level regulations to environmental policy, this is the first time that either a researcher or the EPA has produced a data file with these designations for all four of these criteria pollutants.[2] The regulation file is merged with the 1.75 million plant-level observations from the five Censuses of Manufactures in the 1967–87 period. These censuses contain detailed questions about plants' characteristics (including county of location), input usage, and output. The combined data file is used to relate the growth of employment, investment, and shipments of manufacturers to the federally mandated regulations across the entire country.

The paper's approach overcomes some of the objections to earlier studies of the impact of environmental regulations. First, the preferred specification includes plant fixed effects, industry by period fixed effects, and county by period fixed effects in plant-level models for the growth of employment, investment, and shipments. Consequently, the estimated regulation effects are purged of all permanent plant characteristics that determine growth, all transitory differences in the mean growth of plants across industries, and all transitory determinants of growth that are common to polluters and nonpolluters within a county. These controls are important because this was a period of dramatic changes in the manufacturing sector, including a substantial increase in competition from foreign firms in some industries, a secular movement of plants from the Rust Belt to the South, and two oil price shocks that had differential effects on particular industries and regions.

Second, this paper uses the principal instruments of the Clean Air Act Amendments (CAAAs), the pollutant-specific, county-level attainment/nonattainment designations, as its measures of regulation. These four designations are the "law of the land" and capture the regional

[2] McConnell and Schwab (1990), Henderson (1996), and Becker and Henderson (2000, 2001) use nonattainment status for O_3 but did not collect information on nonattainment status for the other pollutants.

and industry variation that Congress imposed with this legislation.[3] In fact, these designations govern the writing and enforcement of the plant-specific regulations that restrict the behavior of polluters. Moreover, the simultaneous evaluation of all four regulations is important, because many plants emit multiple pollutants and many counties are designated as nonattainment for multiple pollutants. These regulations should address Jaffe et al.'s (1995) criticism that previous studies rely on measures of regulation that are too aggregated (e.g., state-level measures) to detect differences in stringency.

Third, the detailed Census of Manufactures questionnaire allows for an examination of regulation's impact across a number of outcomes and categories of plants. The previous literature generally focuses on the effects of regulation on a single outcome variable (e.g., employment) or on a particular category of plants (e.g., new plants and their location decisions). This narrow focus may provide an incomplete picture of the consequences of environmental regulations. In contrast, this paper examines the impacts of regulation on the growth of employment, capital stock, and shipments. Moreover, its estimates are derived from a sample that includes existing plants as well as newly opened ones.

The results indicate that the CAAAs substantially retarded the growth of polluting manufacturers in nonattainment counties. The estimates suggest that in the first 15 years after the amendments became law (i.e., 1972–87), nonattainment counties (relative to attainment ones) lost approximately 590,000 jobs, $37 billion in capital stock, and $75 billion (1987 dollars) of output in pollution-intensive industries. Importantly, these findings are robust across many specifications, and the effects are evident across a wide range of polluting industries. Although the decline in manufacturing activity was substantial in nonattainment counties, it was modest compared to the size of the entire manufacturing sector.

The paper is organized as follows. Section II describes the statutory requirements of the CAAAs and the variation in regulation that they imposed. Section III describes the data and presents some summary statistics on the regulations' scope. Section IV presents the identification strategy, and Section V discusses the estimation results. Section VI develops two measures of the magnitude of the regulations' impacts and interprets the results. Section VII concludes the paper.

II. The CAAAs and the Variation in Regulation

The ideal analysis of the relationship between industrial activity and environmental regulations involves a controlled experiment in which

[3] A few states and localities (e.g., California) have imposed clean air regulations that are stricter than the federal ones. Any regulations over and above the federally mandated ones are unobserved variables in the subsequent analysis.

environmental regulations are randomly assigned to plants. Then the changes in activity among the regulated and unregulated can be compared with confidence that any differences are causally related to regulation.

In the absence of such an experiment, an appealing alternative is to find a situation in which similar plants face different levels of regulation. The structure of the 1970 and 1977 CAAAs may provide such an opportunity. In particular, the amendments introduce substantial cross-sectional and longitudinal variation in regulatory intensity at the county level. This section describes the CAAAs and why they may offer the opportunity to credibly identify the relationship between environmental regulation and industrial activity.

A. *The CAAAs and Their Enforcement*

Before 1970 the federal government did not play a significant role in the regulation of air pollution; that responsibility was left primarily to state governments. In the absence of federal legislation, few states found it in their interest to impose strict regulations on polluters within their jurisdictions. Disappointed with the persistently high concentrations of CO, O_3,[4] SO_2, and TSPs[5] and concerned about their detrimental health impacts,[6] Congress passed the 1970 Clean Air Act Amendments.[7]

The centerpiece of this legislation is the establishment of separate federal air quality standards for each of the pollutants, which all counties are required to meet. Appendix table A1 lists these air quality standards. The stated goal of the amendments is to bring all counties into compliance with the standards by reducing local air pollution concentrations. The legislation requires the EPA to assign annually each county to either nonattainment or attainment status for each of the four pollutants, on the basis of whether the relevant standard is exceeded.

The CAAAs direct the 50 states to develop and enforce local pollution abatement programs that ensure that each of their counties attains the standards. In their nonattainment counties, states are required to de-

[4] There are separate standards for O_3 and nitrogen dioxide (NO_2), and, in principle, a county could meet one of these standards but not the other. However, O_3 is the result of a complicated chemical process that involves NO_2, and the vast majority of counties that were nonattainment for NO_2 were also nonattainment for O_3. As a result, I designated a county nonattainment for O_3 if the EPA labeled it nonattainment for either O_3 or NO_2. All future references to O_3 refer to this combined measure.

[5] In 1987 the EPA changed its focus from the regulation of all particulates (i.e., TSPs) to the smaller particulate matter (PM10s), which have an aerodynamic diameter equal to or less than 10 micrometers. In 1997 the PM10 regulation was replaced with a PM2.5 one.

[6] See Dockery et al. (1993), Ransom and Pope (1995), and Chay and Greenstone (2002*a*, 2002*b*) on the relationship between air pollution and human health.

[7] See Lave and Omenn (1981) and Liroff (1986) for more detailed histories of the CAAAs.

velop plant-specific regulations for every major source of pollution. These local rules demand that substantial investments, by either new or existing plants, be accompanied by installation of state-of-the-art pollution abatement equipment and by permits that set emissions ceilings. The 1977 amendments added the requirement that any increase in emissions from new investment be offset by a reduction in emissions from another source within the same county.[8] States are also mandated to set emission limits on existing plants in nonattainment counties.

In attainment counties, the restrictions on polluters are less stringent. Large-scale investments require less expensive (and less effective) pollution abatement equipment; moreover, offsets are not necessary. Smaller investments and existing plants are essentially unregulated. Additionally, nonpolluters are free from regulation in both sets of counties.

Both the states and the federal EPA are given substantial enforcement powers to ensure that the CAAAs' intent is met. For instance, the federal EPA must approve all state regulation programs in order to limit the variance in regulatory intensity across states. On the compliance side, states run their own inspection programs and frequently fine noncompliers. The 1977 legislation made the plant-specific regulations both federal and state law, which gives the EPA legal standing to impose penalties on states that do not aggressively enforce the regulations *and* on plants that do not adhere to them. Nadeau (1997) and Cohen (1998) document the effectiveness of these regulatory actions at the plant level. Perhaps the most direct evidence that the regulations are enforced successfully is that air pollution concentrations declined more in nonattainment counties than in attainment ones during the 1970s and 1980s (Henderson 1996; Chay and Greenstone 2000, 2002*a*; Greenstone 2002).

B. Which Industries Are Targeted by the CAAAs?

The manufacturing sector is a primary contributor of the four regulated pollutants. Within this sector, the pollutant-specific regulations apply only to emitters of the relevant pollutants. An official list of the emitting industries is unavailable from the EPA, so it was necessary to develop a rule to divide manufacturers into emitters and nonemitters for each of the four pollutants. It is important that this assignment rule be accurate, because the subsequent analysis compares the growth of emitters and nonemitters, and misclassification will bias the estimated regulation effects.

[8] The reduction in pollution due to the offset must be larger than the expected increase in pollution associated with the new investment. The offsets could be purchased from a different facility or generated by tighter controls on existing operations at the same site (Vesilind, Peirce, and Weiner 1988).

After exploring a number of alternatives, I use the EPA's estimates of industry-specific emissions (see App. table A2) to determine pollutant-specific emitter status. Industries that account for 7 percent or more of industrial sector emissions of that pollutant are designated an emitter; all other industries are considered nonemitters.[9] This rule aims to mimic the EPA's focus on the dirtiest industries in the years in which the CAAAs were first in force. Its application causes 12 separate industries to be designated as emitters of at least one of the pollutants. The subsequent analysis demonstrates that the estimated effects of the regulations are largely insensitive to other reasonable definitions of emitter status.

Under any rule, each industry could emit any of the 16 (i.e., 2^4) possible combinations of the four pollutants. The 7 percent assignment rule divides the manufacturing sector such that eight of the possible combinations are represented. The seven polluting combinations (with the relevant industry names and standard industrial classification [SIC] codes in parentheses) are emitters of O_3 (printing 2711–89; organic chemicals 2861–69; rubber and miscellaneous plastic products 30; fabricated metals 34; and motor vehicles, bodies, and parts 371), SO_2 (inorganic chemicals 2812–19), TSPs (lumber and wood products 24), CO/SO_2 (nonferrous metals 333–34), $CO/O_3/SO_2$ (petroleum refining 2911), O_3/SO_2/TSPs (stone, clay, glass, and concrete 32), and $CO/O_3/SO_2$/TSPs (pulp and paper 2611–31 and iron and steel 3312–13 and 3321–25). The EPA's estimates of emissions indicate that the remaining industries are not major emitters of any of the four pollutants, and I assign these industries to the clean category.[10]

C. *Summarizing the Variation in Regulation Due to the CAAAs*

The structure of the CAAAs provides three sources of variation in which plants were affected by the nonattainment designations. This subsection summarizes this variation and highlights its importance from an eval-

[9] See the Data Appendix for further details on the determination of pollutant-specific emitting status.

[10] It is informative to compare this division of the manufacturing sector into polluters and nonpulluters with those in the previous literature. In each of their papers, Henderson (1996) and Becker and Henderson (2000, 2001) designate different sets of industries as subject to O_3 nonattainment status. The current paper's set of ozone emitters spans the intersection of their three sets, with the exception that the 7 percent rule excludes wood furniture (SIC 2511) and plastic materials and synthetics (SIC 282). Berman and Bui's (1998, 2001) list of regulated industries is not readily comparable with this paper's list for at least two reasons. First, their list is not pollutant-specific. Second, their papers examine local regulations in the South Coast Air Basin that are over and above federal and state regulations, so their set of regulated industries is likely to be broader than those scrutinized by the federal EPA. Nevertheless, there is substantial overlap between their list of industries targeted in the South Coast and the industries that are classified as emitters of at least one pollutant by this paper's assignment rule.

uation perspective. It also briefly discusses some of the sources of this variation and why they may reinforce the credibility of the subsequent analysis.

The first dimension of variation is that at any point in time the pollutant-specific nonattainment designations are reserved for counties whose pollution concentrations exceed the federal standards. This cross-sectional variation allows for the separate identification of industry-specific shocks and the regulation effects. This may be especially important in the 1967–87 period, because there were dramatic shocks (e.g., oil crises, recessions, and increases in foreign competition) that affected industries differentially.

The second dimension of variation is that a county's attainment/nonattainment designations vary over time as its air quality changes. Consequently, individual plants might be subject to regulations in one period but not in a different one. This longitudinal variation allows for the inclusion of plant fixed effects in equations for plant-level growth. Consequently, the paper presents estimated regulation effects that are derived from within-plant comparisons under the attainment and nonattainment regulation regimes.

The third dimension of variation is that within nonattainment counties, only plants that emit the relevant pollutant are subject to the regulations. This intracounty variation allows for estimation of models that include unrestricted county by period effects so that time-varying factors common to all plants *within* a county are not confounded with the effects of regulation. For example, the 1980–82 recession caused polluting and nonpolluting manufacturers in Allegheny County, Pennsylvania (i.e., Pittsburgh), to reduce their operations. Since Allegheny County was designated nonattainment for all four pollutants at this time, this decline would be falsely attributed to the regulations if the intracounty variation in emitting status were unavailable.

Some of the sources of variation in nonattainment status reinforce the credibility of an evaluation based on the CAAAs. Specifically, the county-level nonattainment designations are federally mandated and therefore may be unrelated to differences in tastes, characteristics, or underlying economic conditions across counties. Moreover, the nonattainment designations depend on whether local pollution levels exceed the federal standards. And while pollution levels are not randomly assigned, scientific evidence suggests that during the years under study, many counties were designated nonattainment because of pollution that was related to weather patterns—a factor that is unlikely to be related to local manufacturing sector activity.[11]

[11] Cleveland et al. (1976) and Cleveland and Graedel (1979) document that wind patterns often cause air pollution to travel hundreds of miles and that the concentration of

III. Data Sources and Summary Statistics

This section comprises four subsections. The subsequent analysis is based on the most comprehensive data available on manufacturing activity and clean air regulations, and subsection *A* describes the sources and structure of these data. Subsection *B* documents the scope of the regulatory program both geographically and within the manufacturing sector. Subsection *C* examines whether nonattainment status is orthogonal to observable determinants of plant growth. Subsection *D* explores whether nonattainment status covaries with county shocks that affect emitters and nonemitters.

A. Data Sources and Structure

The manufacturing data come from the micro data underlying the five quinquennial Censuses of Manufactures from 1967 to 1987. In each census a plant observation contains information on employment, capital stock, total value of shipments, age, whether it is part of a multiunit firm, and whether the observation is due to a survey response or derived from an administrative record. The four-digit SIC code and county of location allow the data on which pollutants are emitted and nonattainment designations to be merged. Importantly, the censuses contain a unique plant identifier, making it possible to follow individual plants over time.[12]

I linked consecutive Censuses of Manufactures to create four periods: 1967–72, 1972–77, 1977–82, and 1982–87. A plant observation in an individual period includes information from the censuses at the beginning and end of the period.[13] Plants that appear in the first census of a period but not in the last are considered "deaths"; analogously, plants that appear in the last but not in the first are designated "births." Plants that appear in both censuses of a period are labeled "stayers."[14] There are 1,737,753 plant observations in these four periods.

O_3 in the air entering the New York region in the 1970s often exceeded the federal standards. Figure 2 below graphically depicts the counties that were designated nonattainment for O_3 and reveals that virtually the entire Northeast, even counties without substantial local production of O_3, is O_3 nonattainment for at least one period. It is evident that this region's nonattainment designations partially reflect its location downwind from heavy O_3 emitters in the Ohio Valley.

[12] See the appendix in Davis, Haltiwanger, and Schuh (1996) for a more thorough description of these data.

[13] Approximately 0.5 percent of plants change SIC codes in a period. Plants are equally likely to switch into and out of emitting industries, so it does not appear that they alter their SIC code to evade regulation.

[14] The permanent plant identifier and the criteria specified by Davis et al. (1996) are used to determine whether a period-specific plant observation qualifies as a birth, death, or stayer. The distribution of plants across these categories is 29 percent births, 27 percent deaths, and 44 percent stayers.

Each of the 3,070 counties is assigned four pollutant-specific attainment / nonattainment designations in every period. A county's pollutant-specific designation in a given period is based on its attainment / nonattainment status in the *first* year of that period (e.g., 1982 determines the regulatory status for the 1982–87 period). All counties are attainment for the four pollutants in the 1967–72 period because the CAAAs were not in force until the end of this period. The attainment / nonattainment designations for the 1977–82 and 1982–87 periods are obtained from the list of nonattainment counties published in the *Code of Federal Regulations* (CFR) in the first year of those periods.[15] The CFR does not list the identity of the nonattainment counties in the early 1970s, and the EPA does not maintain a historical record of them. Consequently, I filed a Freedom of Information Act request and obtained data from the EPA's national pollution monitoring network for these years. For the 1972–77 period, I consider a county nonattainment for a pollutant if it had a pollution monitor reading that exceeded the relevant federal standard in 1972. The Data Appendix provides more details on the determination of nonattainment / attainment status.

There are at least two reasons that this definition of the regulation variables is preferable to alternatives based on nonattainment status later in a period. First, it is unlikely that plants can quickly change their production processes in response to regulation. Second, Berman and Bui (1998, 2001) document that the plant-level regulations associated with nonattainment status often set compliance dates a number of years in advance.[16]

B. The Incidence and Geographic Scope of the Nonattainment Designations

Table 1 reports summary information on the incidence of the pollutant-specific nonattainment designations. Column 1 lists the number of counties designated nonattainment for each pollutant, period by period. It is apparent that the regulatory programs for O_3 and TSPs are the most pervasive.

Column 2 details the number of counties that switch from attainment to nonattainment between periods, and column 3 enumerates the

[15] The publication of nonattainment counties in the CFR begins in 1978, so this year determines the designations for the 1977–82 period.

[16] The determination of nonattainment status from a single year might cause measurement error in the regulation variables, leading to attenuation bias in the estimated effects of regulation. In order to explore this possibility, I experimented with designating a county nonattainment if it received this designation in the first or second year of a period or the year before a period begins. (In the case of the 1982–87 period, this is 1981, 1982, or 1983.) I also used as a measure of regulation the total number of years during the period in which the county is designated nonattainment. The paper's findings are unchanged when nonattainment status is assigned in these alternative ways.

TABLE 1
INCIDENCE and Changes in Nonattainment Status

	Nonattainment Period *t* (1)	Attainment Period *t*ff 1 and Nonattainment Period *t* (2)	Nonattainment Period *t*ff 1 and Attainment Period *t* (3)
	A. Carbon Monoxide (CO)		
1967–72	0	0	0
1972–77	81	81	0
1977–82	144	90	27
1982–87	137	15	22
	B. Ozone (O_3)		
1967–72	0	0	0
1972–77	32	32	0
1977–82	626	595	1
1982–87	560	104	170
	C. Sulfur Dioxide (SO_2)		
1967–72	0	0	0
1972–77	34	34	0
1977–82	87	75	22
1982–87	60	7	34
	D. Total Suspended Particulates (TSPs)		
1967–72	0	0	0
1972–77	296	296	0
1977–82	235	108	169
1982–87	176	24	83

NOTE.—There are 3,070 counties in the Census of Manufactures data files. See the Data Appendix for a description of how the pollutant-specific nonattainment designations are assigned.

changes from nonattainment to attainment. It is evident that there is substantial movement into and out of nonattainment status between periods. For example, of the 945 counties that are designated nonattainment for at least one of the pollutants, only 21 retain the same designations for all four pollutants throughout the three periods in which the CAAAs are in force. These changes in regulatory status reflect a number of factors, including the EPA's increasing awareness of which counties exceeded the federal standards (e.g., the large increase in the number of nonattainment counties between 1972–77 and 1977–82, particularly in the case of ozone), air quality improvement in nonattainment counties, and deterioration in attainment ones. This intercounty variation in nonattainment status is important for identification purposes because it allows for the inclusion of county or plant fixed effects in the econometric models.

Figures 1–4 graphically summarize the incidence of the four nonattainment designations. The shading indicates the number of periods a county is designated nonattainment for the relevant pollutant: white for zero, light gray for one, gray for two, and black for three. By moving

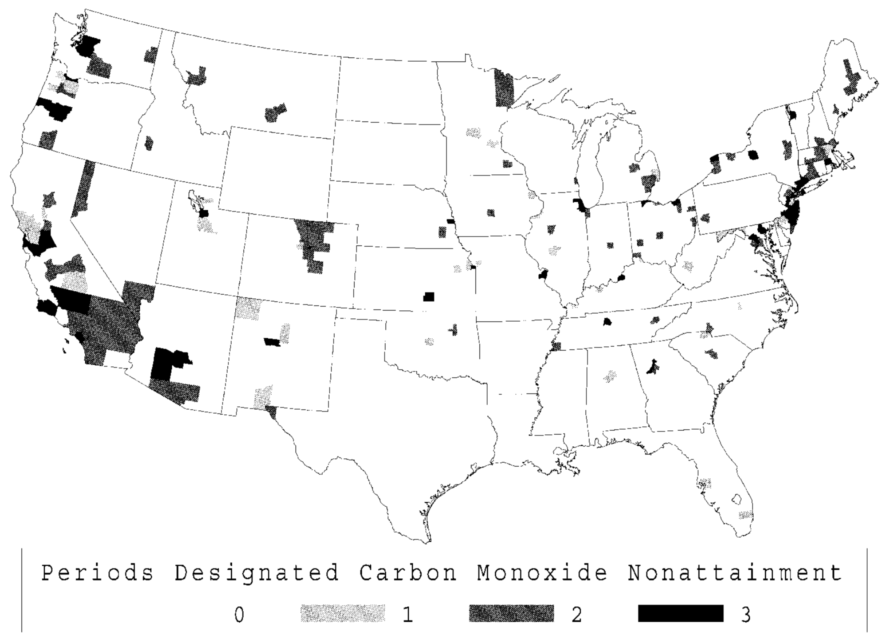

FIG. 1.—Incidence of nonattainment for carbon monoxide by county (1972–77, 1977–82, and 1982–87). Source: EPA Air Quality Subsystem Database, *Code of Federal Regulations* (various issues).

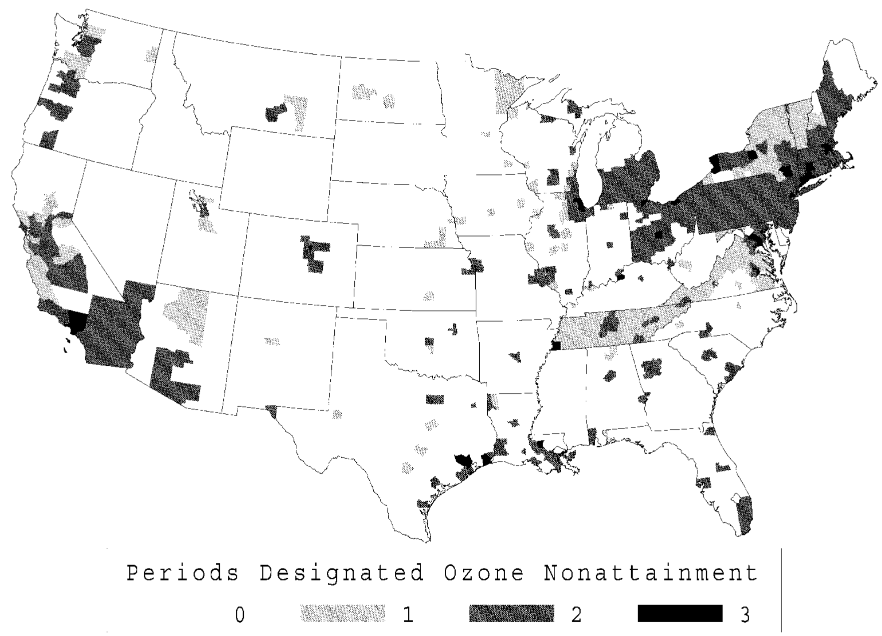

FIG. 2.—Incidence of nonattainment for ozone by county (1972–77, 1977–82, and 1982–87). Source: EPA Air Quality Subsystem Database, *Code of Federal Regulations* (various issues).

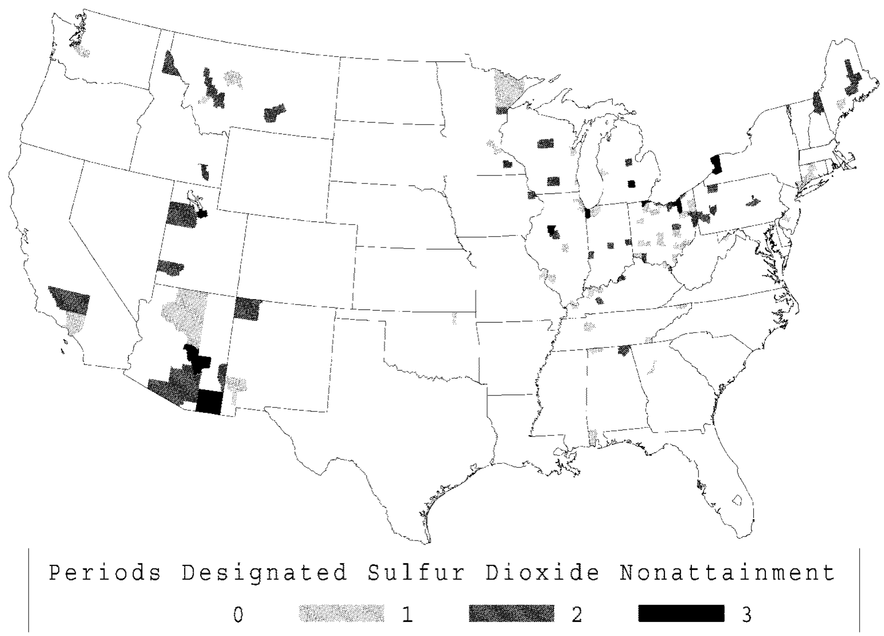

FIG. 3.—Incidence of nonattainment for sulfur dioxide by county (1972–77, 1977–82, and 1982–87). Source: EPA Air Quality Subsystem Database, *Code of Federal Regulations* (various issues).

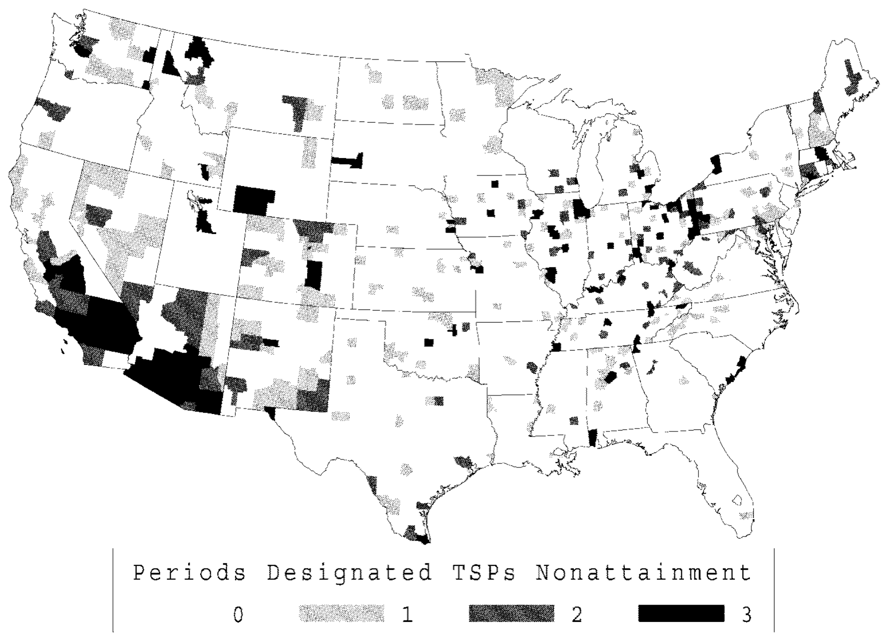

FIG. 4.—Incidence of nonattainment for total suspended particulates by county (1972–77, 1977–82, and 1982–87). Source: EPA Air Quality Subsystem Database, *Code of Federal Regulations* (various issues).

TABLE 2
Manufacturing Employment, by Pollutant Emitted and Pollutant-Specific Attainment Status

	1967–72 (1)	1972–77 (2)	1977–82 (3)	1982–87 (4)
CO-emitting plants	1,111,534	1,040,563	951,515	744,061
CO attainment	1,111,534	839,456	648,526	517,767
CO nonattainment	…	201,108	302,989	226,294
O_3-emitting plants	5,453,418	5,581,151	5,542,548	5,412,151
O_3 attainment	5,453,418	5,108,078	1,294,500	1,492,627
O_3 nonattainment	…	473,073	4,248,048	3,919,524
SO_2-emitting plants	1,783,243	1,717,904	1,598,742	1,358,083
SO_2 attainment	1,783,243	1,468,781	1,233,592	1,170,479
SO_2 nonattainment	…	249,123	365,150	187,604
TSPs-emitting plants	2,101,561	2,071,924	1,899,173	1,697,843
TSPs attainment	2,101,561	1,303,442	1,114,749	1,160,430
TSPs nonattainment	…	768,482	784,424	537,413
Total manufacturing sector	17,438,187	17,350,726	17,521,355	17,100,413

Note.—See the note to table 1. Employment is the mean of total employment in the first and last years of each five-year period covered by the 1967–87 Censuses of Manufacturers.

back and forth between the maps, one can see that many counties were regulated for more than one pollutant (e.g., parts of southern California, Arizona, and the Rust Belt). The national scope of the regulatory programs is also evident: all 48 continental states have at least one nonattainment county.[17]

Table 2 presents the levels of employment for emitters of each of the pollutants and the entire manufacturing sector in the four periods.[18] The level is calculated as the mean of the levels in the first and last years of a period. The table also separately lists employment in nonattainment and attainment counties within the four categories of emitters by period.

The portion of the manufacturing sector that is an emitter varies across the pollutants. For instance, O_3 emitters account for the largest share (roughly 31.7 percent) of total manufacturing employment. The shares for the other polluting industries are 11.2 percent for TSPs, 9.3 percent for SO_2, and 5.5 percent for CO. Although they are not shown in table 2, the ranges for capital stock and shipments are 19.9 percent (TSPs emitters) to 46.2 percent (O_3 emitters) and 10.7 percent (TSPs emitters) to 37.9 percent (O_3 emitters), respectively. Regardless of the measure, it is apparent that the emitting industries account for a substantial proportion of the manufacturing sector.

[17] Alaska and Hawaii are excluded from the analysis.

[18] Many plants emit multiple pollutants, so the pollutant-specific rows (e.g., CO-emitting plants) of table 2 are not mutually exclusive. Consequently, summing across the rows within a single period overstates employment in plants that emit any pollutant in that period.

Table 2 also documents that within the four sets of emitting plants, a meaningful share of employment is located in both attainment and nonattainment counties. Consequently, it may be possible to obtain precise estimates of the effects of the pollutant-specific nonattainment designations. Finally, the level of employment in emitting industries located in nonattainment counties is a summary measure of the size of the group that was potentially affected by these designations.

C. Is Nonattainment Status Orthogonal to Observable Determinants of Plant Growth?

In the ideal case, nonattainment status would be orthogonal to all determinants of plant growth. The regulation effects could then be calculated by a simple comparison of mean growth rates in the two sets of counties.

While it is impossible to make statements about unobserved covariates, it is instructive to compare observable ones in nonattainment and attainment counties. If the observable covariates are balanced across the two sets of counties, then the unobservables may be more likely to be balanced (Altonji, Elder, and Taber 2000). Further, consistent inference does not rely on functional form assumptions about the relationship between the observables and plant growth when the observable determinants are balanced. To the extent that the observables are unbalanced, these comparisons will identify likely sources of bias and inform the choice of statistical model.

Table 3 displays the means of determinants of plant growth within three categories of counties. These categories comprise counties that are attainment for CO in the 1972–77 period (col. 1*a*), attainment for CO in 1972–77 but CO nonattainment in a later period (col. 1*b*), and CO nonattainment in 1972–77 (col. 2). Panel A of the table presents means of county-level covariates, and panel B documents means of the characteristics of CO-emitting plants. The comparison of 1972–77 CO nonattainment and attainment counties is only one of the comparisons that underlie the subsequent analysis, but it captures many of the themes that are present in comparisons of nonattainment and attainment counties in different periods and for different pollutants.

Inspection of columns 1*a* and 2 provides a comparison of all CO attainment counties with CO nonattainment counties in the 1972–77 period. It is evident that both the county-level and plant-level characteristics differ with nonattainment status. In particular, nonattainment counties have higher population densities, rates of urbanization, average education levels, per capita income, and per capita government revenues. Moreover, a smaller fraction of their jobs are in the manufacturing sector, and they have lower poverty rates. Importantly, the average num-

TABLE 3
MEANS of County and Plant Characteristics by 1972–77 CO Nonattainment Status

	CO Attainment, 1972–77 (1*a*)	CO Attainment, 1972–77, and CO Nonattainment, 1977–82 or 1982–87 (1*b*)	CO Nonattainment, 1972–77 (2)
	A. County Characteristics in 1970		
Number of counties	2,989	100	81
Population	47,157	395,376	620,654
Population density	1,826	6,354	4,868
% urban	.65	.90	.94
% ≥12 years of education	.50	.55	.57
% ≥16 years of education	.10	.11	.13
% employment in manufacturing	.262	.266	.242
Unemployment rate	.044	.045	.046
Poverty rate	.119	.082	.081
Income per capita (1982–84 dollars)	7,456	8,712	9,414
Per capita government revenues	248	296	403
	B. CO-Emitting Plant Characteristics in 1972		
Number of CO-emitting plants	1.0	6.8	14.2
Average employment	269	362	175
% operating at least 10 years	55.2	59.3	51.3
% part of multiunit firm	34.6	40.7	40.1

NOTE.—See the note to table 1. All entries are averages across counties in the relevant category. The data on county characteristics are derived from the 1970 Census. The 1972 Census of Manufactures is used to determine the means of CO-emitting plant characteristics. The entries in col. 1*a* are calculated from the 2,989 counties that are designated CO attainment in the1972–77 period, and the sample in col. 2 comprises the 81 counties that are CO nonattainment in the same period. Col. 1*b* is the subset of the col. 1*a* counties that are CO attainment in 1972–77 and CO nonattainment in at least one of the 1977–82 and 1982–87 periods.

ber of CO-emitting plants is substantially higher in nonattainment counties (14.2) than in attainment counties (1.0). Further, CO-emitting plants in nonattainment counties are younger, more likely to be part of a multiestablishment firm, and smaller (as measured by employment).

An alternative to forming the "counterfactual" from all CO attainment counties is to restrict this group to counties that are CO attainment in 1972 but CO nonattainment in later periods. A statistical model that includes county fixed effects effectively refines the counterfactual group in this way. Columns 1*b* and 2 permit an exploration of the similarity of these two sets of counties. It is evident that this subset of 1972–77 CO attainment counties is more similar to the nonattainment counties than the unrestricted set of attainment counties was. For example, the means of the population density, level of education, income per capita, and poverty rate in column 1*b* are all closer to the means of these variables in nonattainment counties. However, the average number of CO-emitting plants and the mean characteristics of these plants differ across these columns.

It is apparent that nonattainment status is not orthogonal to observable county- or plant-level determinants of plant growth in either set of attainment counties. Moreover, it is plausible that the same is true for unobservable characteristics. It will be necessary to estimate statistical models that attempt to control for these differences to obtain consistent estimates of the regulation effects.

D. Do Countywide Shocks Covary with Nonattainment Status?

This subsection explores the validity of the assumption that nonattainment status is orthogonal to county-specific determinants of growth that are common to polluters and nonpolluters. This identifying assumption is pervasive in the previous literature (e.g., Bartik 1985; Barbera and McConnell 1986; McConnell and Schwab 1990; Henderson 1996; Levinson 1996; Berman and Bui 1998, 2001; Becker and Henderson 2000, 2001). For brevity I focus on the case in which the dependent variable is the percentage growth in plant employment, but the findings are similar for capital stock and shipments.[19]

Table 4 presents two estimates of the effect of the regulation of each pollutant on employment growth. The first estimate is derived from a sample that is limited to plants that emit the relevant pollutant and is contained in column 1. The column 2 estimate is obtained from all 1,620,942 plant observations with nonmissing employment growth. In both cases the reported parameter is taken from an indicator that is equal to one if the county is nonattainment for the specified pollutant and the plant is an emitter of that pollutant.

The regressions control for a number of plant-level variables that the next section describes in greater detail. Additionally, the two specifications include county fixed effects and industry by period indicators. For the column 1 specification's estimated regulation effect to be unbiased, it is necessary to assume that the regulation of that pollutant is the *only* county-level determinant of employment growth that differs between nonattainment and attainment counties. In contrast, the column 2 specification controls for unobserved, permanent county-level determinants of growth common to emitters and nonemitters.

A comparison of the estimates in columns 1 and 2 provides an in-

[19] The percentage growth is calculated as the change in plant employment between t and t ff 5, divided by the mean of the t and t ff 5 levels. Section IV provides more details about this measure of percentage change.

TABLE 4

ESTIMATED Regression Models for the Percentage Change in Employment with One Regulation Effect per Regression

	Carbon Monoxide		Ozone		Sulfur Dioxide		Total Suspended Particulates	
	CO Emitters (N p 14,456) (1)	All Plants (N p 1,620,942) (2)	O_3 Emitters (N p 543,121) (1)	All Plants (N p 1,620,942) (2)	SO_2 Emitters (N p 99,854) (1)	All Plants (N p 1,620,942) (2)	TSPs Emitters (N p 257,135) (1)	All Plants (N p 1,620,942) (2)
CO regulation effect	ff .041 (.040)	ff .074 (.031)						
O_3 regulation effect			.068 (.011)	.025 (.009)				
SO_2 regulation effect					ff .049 (.030)	ff .040 (.027)		
TSPs regulation effect							ff .021 (.017)	ff .016 (.014)
R^2	.127	.100	.112	.100	.095	.100	.121	.100

NOTE.—The entries are taken from regressions in which the dependent variable is the change in plant employment between *t* and *t* ff 5, divided by the mean of the *t* and *t* ff 5 levels. The equations are weighted by the denominator of the dependent variable. All specifications include county fixed effects and industry by period indicators. Heteroskedastic-consistent standard errors are reported in parentheses.

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formal test of this assumption. The estimates will differ if nonemitters' growth rate covaries with nonattainment status. The regulation effects for SO_2 and TSPs are similar in the two columns. However, the regulation effects for CO and O_3 in column 1 appear to be biased upward. Most dramatically, the column 1 O_3 estimate suggests that nonattainment status at the beginning of a period is associated with a 6.8 percent increase in employment in O_3-emitting industries five years later. Since pollution can be modeled as an input and regulation as a tax on pollution, standard neoclassical models predict an ambiguous effect on demand for other inputs (e.g., labor). Nevertheless, such a large, positive effect is surprising. In column 2, the estimated regulation effect for O_3 shrinks to 2.5 percent, demonstrating the importance of allowing for county-specific factors common to emitters and nonemitters.

It is evident that in the case of CO and O_3, nonattainment status is *not* orthogonal to county-level shocks to growth. The next section describes the preferred statistical models and explains how they try to purge the likely sources of bias.

IV. Identification Strategy

In order to explore more rigorously the effects of the nonattainment designations on the growth of manufacturers' activity, the plant-level data are fit to the following equation:

$$
\begin{aligned}
\%DE_{pt}\ p\ & \frac{E_{pt}\ ff\ E_{ptff\,5}}{(E_{pt}\ ffi\ E_{ptff\,5})/2} \\
& p\ b_1\mathbf{X}_{ptff\,5}\ ffi\ b_{2t}\mathbf{ind}_i\ ffi\ b_{3t}\mathbf{nonattain}_{ctff\,5} \\
& \quad ffi\ b_4 1(\text{emit CO } p\ 1\ \&\ \text{nonattain CO } p\ 1)_{citff\,5} \\
& \quad ffi\ b_5 1(\text{emit } O_3\ p\ 1\ \&\ \text{nonattain } O_3\ p\ 1)_{citff\,5} \\
& \quad ffi\ b_6 1(\text{emit } SO_2\ p\ 1\ \&\ \text{nonattain } SO_2\ p\ 1)_{citff\,5} \\
& \quad ffi\ b_7 1(\text{emit TSPs } p\ 1\ \&\ \text{nonattain TSPs } p\ 1)_{citff\,5}\ ffi\ De_{pt},
\end{aligned}
$$

where $De_{pt}\ p\ a_p\ ffi\ g_{ct}\ ffi\ Du_{pt}$. Here p indexes a plant, c references county, i indexes industry, and t and $t\,ff\,5$ index the last and first years of a period, respectively. The term $\%DE_{pt}$ is the dependent variable (i.e., employment, capital stock, and the value of shipments) and is measured as the percentage change between t and t ff 5.[20] The term De_{pt} is the stochastic error term. Equation (1) is weighted by the denominator of the dependent variable to account for differences in cell size.

The term $\mathbf{X}_{ptff\,5}$ is a vector of variables, calculated at t ff 5 so that they are "pretreatment." There are indicators for four categories of plant size based on shipments (i.e., smaller than the median, between the median and the seventy-fifth percentile, between the seventy-fifth percentile and the mean, and greater than the mean); whether the plant has operated for at least 10 years; ownership by a firm with multiple establishments; and whether the observation is a response to the Census Bureau questionnaire or is derived from federal administrative records. Previous research shows that these variables are important determinants of plant-level growth (Dunne, Roberts, and Samuelson 1989*a*, 1989*b*; Davis and Haltiwanger 1992). The vector $\mathbf{X}_{ptff\,5}$ also contains the average industry-specific wage in the plant's county as a measure of labor costs and the number of employees at other plants in the same industry within the same county to adjust for agglomeration effects (Krugman 1991).

The term $\mathbf{ind}_i$ is a vector of industry indicator variables whose effects are allowed to vary by period. In most of the subsequent analysis, there are 13 industry indicators: one for each of the 12 industries that are classified as an emitter of at least one of the four regulated pollutants and one for the remaining "clean" industries. These variables nonparametrically absorb all time-varying industry-level unobservables at the level at which the regulations are applied. Further, the $\mathbf{nonattain}_{ctff\,5}$ vector contains a separate dummy variable for each of the four pollutant-specific nonattainment designations. These dummies control for unobserved factors that equally affect polluting and nonpolluting plants in nonattainment counties. Their effect is also allowed to vary by period.

The parameters b_4–b_7 capture the variation in the dependent variables specific to polluting plants (relative to nonpolluters) in nonattainment counties (relative to attainment ones). These parameters provide estimates of the mean effect of the pollutant-specific regulations on the plants that are directly targeted by them. Henceforth, they are referred to as the "regulation effects." An attractive feature of this specification is that, in contrast to the previous literature, each of the estimated

[20] This measure of percentage change is an alternative to the difference of the natural logarithms of the year t and t ff 5 levels. It is a second-order approximation to the ln difference measure, ranges from ff 2.0 to ffi2.0, and portrays expansion and contraction symmetrically (Davis et al. 1996). Importantly, it allows the sample to contain observations on "births" and "deaths," i.e., plants that do not operate in either the first or last year of a period. A comparison of the results from a sample of "stayers" reveals that the estimated regulation effects are nearly identical when the dependent variable is calculated as the ln difference.

regulation effects is obtained while holding the others constant.[21] This is relevant because many plants were subject to more than one of the nonattainment designations.[22]

Prior research indicates that there are important permanent and transitory regional determinants of manufacturing activity.[23] There are a number of ways to model these factors with the available data. One possibility is to include county fixed effects so that counties that were never designated nonattainment for a particular pollutant do not help identify the parameters of interest. In this case, the pollutant-specific regulation effects are estimated from 189 (CO), 730 (O_3), 134 (SO_2), and 436 (TSPs) counties.

As the specification of De_{pt} indicates, another possibility is to include a full set of fixed effects for the more than 735,000 plants in the sample and county by period indicators. The plant fixed effects greatly reduce the degrees of freedom, but they control for differences in permanent plant growth rates that might be correlated with nonattainment status. Such a correlation might occur if nonattainment counties provide the conditions necessary for emitting plants or industries to flourish (e.g., easy access to the interstate highway system, a workforce that suits their technology, or proximity to a natural resource). In this specification, the regulation effects are identified from within-plant comparisons of growth rates under the nonattainment and attainment regimes. The county by period indicators nonparametrically adjust for time-varying shocks to growth common to emitters and nonemitters within the same county.

In the subsequent tables, heteroskedastic-consistent standard errors of the regression parameters are reported (White 1980). Since the data are taken from censuses, the standard errors' interpretation is not straightforward. On the one hand, the sample includes all the members of a finite population, so the standard errors need not be calculated. On the other hand, the observed finite population can be considered

[21] I also experimented with including the 12 "cross-pollutant" interactions (e.g., 1(emit O_3 p 1 & nonattain CO p 1) in the specification. Across the dependent variables and specifications, the hypothesis that they are jointly equal to zero is generally not rejected by a χ^2 test at standard confidence levels. Moreover, in these plant-level regressions, their inclusion does not substantially alter the estimates of the four regulation effects. Notably, the cross-pollutant interactions are more important in grouped regressions and with stricter definitions of emitter status, as in Greenstone (1998).

[22] McConnell and Schwab (1990), Henderson (1996), and Becker and Henderson (2000, 2001) use the equivalent of the O_3 nonattainment designation but restrict the effect of the other pollutant-specific designations to equal zero. The remainder of the literature uses regulatory measures that do not account for the pollutant-specific nature of the CAAAs.

[23] Bartik (1985) and Holmes (1998) show that a number of local factors including unionization density, tax rates, the provision of public services, and right-to-work laws affect firms' investment decisions. Moreover, Blanchard and Katz (1992) demonstrate that shocks to regions' growth rates can persist for as long as a decade.

a member of an unobserved superpopulation; thus the standard errors associated with regression parameters have their usual interpretation.

In summary, the estimated regulation effects are purged of many likely sources of bias. For example, the specification that includes plant fixed effects, county by period indicators, and industry by period dummies is robust to all unobserved permanent determinants of plant growth, all unobserved transitory factors common to polluting and nonpolluting plants within a county, and all unobserved industry-specific shocks to growth. However, the estimated regulation effects are not robust to transitory determinants of growth specific to emitting industries (or plants) located in counties that are nonattainment for the emitted pollutant(s). In other words, county by industry and county by plant shocks to growth are potential sources of bias.

V. The Amendments' Impact on Manufacturing Sector Activity

This section is divided into three subsections. Subsection *A* presents the estimated effects of the regulations on the growth rates of employment, shipments, and capital from fitting the preferred specifications discussed in Section IV. Subsection *B* tests for heterogeneity in the regulation effects across industries. Subsection *C* probes the robustness of the results.

A. The Effects of the CAAAs on Manufacturing Activity

In a standard neoclassical model in which pollution, labor, and capital are inputs in the production process, the predicted effect of regulation, which increases the price of pollution, on labor and capital demand is ambiguous. The theoretical prediction on output is unambiguously negative. This subsection tests these predictions.

Total Employment

Table 5 presents the employment results from the estimation of equation (1), using data from all plant observations over the four periods. The columns correspond to specifications that include additional sets of controls as one reads from left to right; the exact controls are noted at the bottom of the table. The mean five-year growth rate of total employment is ff 1.4 percent.

The specification in column 1 includes industry by period fixed effects and allows the effect of nonattainment status to vary by period. Here, the estimated regulation effects are derived from comparisons between all attainment and nonattainment counties.

The results in column 1 suggest that nonattainment status modestly

TABLE 5
ESTIMATED Regression Models for the Percentage Change in Employment

	(1)	(2)	(3)	(4)
CO regulation effect (b_4)	ff .084	ff .075	ff .086	ff .163
	(.032)	(.031)	(.030)	(.045)
O_3 regulation effect (b_5)	.001	.022	ff .011	ff .049
	(.011)	(.010)	(.010)	(.015)
SO_2 regulation effect (b_6)	ff .004	ff .016	.003	.001
	(.029)	(.028)	(.029)	(.036)
TSPs regulation effect	ff .024	ff .010	ff .020	ff .024
(b_7)	(.014)	(.013)	(.013)	(.024)
R^2	.109	.119	.144	.504
Industry by period fixed effects	yes	yes	yes	yes
Nonattainment by period fixed effects	yes	yes	no	no
County fixed effects	no	yes	no	no
County by period fixed effects	no	no	yes	yes
Plant fixed effects	no	no	no	yes

NOTE.—See the note to table 4. In all specifications, the sample includes the 1,620,942 plant observations with nonmissing and nonnegative employment levels. The mean five-year growth rate of employment in the sample is ff 1.4 percent.

retards the growth of employment. The estimates indicate that a CO nonattainment designation at the beginning of a period is associated with an 8.4 percent reduction in employment levels in CO-emitting plants five years later. This estimate would be judged statistically significant at conventional levels. The regulation effect for TSPs is ff 2.4 percent and would be considered significant at the 10 percent level but not by stricter criteria. In contrast, O_3 and SO_2 nonattainment statuses are basically uncorrelated with the respective growth of emitters of those pollutants. Interestingly, the estimated regulation effects for O_3 and SO_2 differ from the estimates that did not account for the effects of the other nonattainment designations as in table 4.

Columns 2 and 3 report the results from adding county fixed effects and county by period effects to the specification, respectively. In both cases, F-tests easily reject the null that the additional parameters are jointly equal to zero. As discussed above, the regulation effects from the specification in column 2 are due to comparisons of counties that experience a change in attainment status over the course of the sample. The estimates in column 3 are based on comparisons between emitters and nonemitters within nonattainment counties. In light of the differences in these first three specifications, it is striking that the estimated regulation effects are essentially the same across the columns.

The specification that requires the least restrictive assumptions for unbiasedness of the regulation effects is the one in column 4, which includes a full set of plant fixed effects. All permanent differences in plant growth rates are controlled for here. As evidenced by the marked increase in the R^2 statistic (.504 compared to .144), the "fit" of the regression is substantially greater. However, an F-test fails to reject the null that the plant fixed effects are jointly equal to zero. This "overparameterization" explains the increased standard errors of the four regulation effects.

The intent in estimating this model is to probe the robustness of the estimated regulation effects from columns 1–3. In this specification, two of the regulation effects imply a larger negative effect on employment and two are essentially unchanged relative to the other specifications. In particular, CO nonattainment status at the beginning of a period is associated with a 16.3 percent decline in employment in CO-emitting plants by the end of the period. The magnitude of the regulation effect for O_3 is larger, and the estimate is now ff 0.049; moreover, it would be judged statistically significant at standard levels. The increased magnitude of these two regulation effects is consistent with the notion that CO and O_3 nonattainment counties offer competitive advantages to emitters of these pollutants. In contrast, the regulation effects for SO_2 and TSPs are essentially unchanged from the other specifications.[24]

Capital Stock

The last subsection documented a robust negative correlation between nonattainment status and employment growth. Here, I explore whether nonattainment status is associated with the capital stock growth rate. Investment may be particularly sensitive to regulation because it reflects plants' conjectures about future profitability. Although it is difficult for plants to adjust their capital stock in the short run, the length of time between observations (five years) means that any impact of regulation should be apparent. In particular, it is likely that five years is enough time for establishments to bring new investments "on line," to substantially reduce their capital stock through depreciation,[25] to open new plants, or to cease operations. Interestingly, the previous literature finds

[24] It is thought that environmental regulations weaken polluters' competitive position by causing them to hire additional nonproduction workers (e.g., engineers or environmental compliance officers) that aid in ensuring adherence to the regulations but do not directly contribute to the production of the firm's output. I examined this hypothesis and found that the regulations' effects were approximately equal across production and nonproduction workers. In other words, these data do not support this hypothesis.

[25] Dixit and Pindyck (1994) show that the sunk cost nature of many investments combined with uncertainty about the future may make it more profitable for a firm to respond to a large negative shock by allowing its capital stock to depreciate, rather than by ceasing operations.

that environmental regulations are not a significant deterrent to new investment in plants and equipment.[26]

There are at least three limitations to the Census of Manufactures data on capital stock. First, the censuses' measure of capital stock comprises productive capital *and* potentially "nonproductive" pollution abatement equipment that is mandated by the regulations. This combined measure may cause the estimated regulation effects to be biased upward, relative to the preferred measure of productive investment.[27] Second, the book value method is used to measure capital stock, which likely overstates the importance of recent investment relative to a perpetual inventory measure.[28] Third, the capital stock measure does not allow for a test of whether the regulations cause plants to change the rate of new investment or affect the value of existing capital. A measure of capital stock that separates new investment from the depreciation/retirement of existing capital would allow for a more nuanced analysis.

Panel A of table 6 presents estimates of the impact of the nonattainment designations on capital stock accumulation. The mean five-year growth rate of capital stock when the book value method is used is 36.5 percent. The columns correspond to specifications that include additional sets of controls as in table 5.

Across the specifications, the capital stock estimates suggest that nonattainment status retards investment, but the evidence is less decisive than in the employment regressions. Similarly to the employment results, the estimated regulation effects are roughly constant across the first three specifications. The commonality of these estimates is especially apparent in the context of the standard errors. The estimates indicate that the effect of the nonattainment designations on capital stock ranged from small and positive (TSPs) to somewhat large and negative (CO and SO_2). However, the regulation effect for CO in column 3 is the only one that would be judged statistically different from zero.

The addition of plant fixed effects in column 4 greatly increases the

[26] A review article concludes that "environmental regulations do not deter investment to any statistically or economically significant degree" (Levinson 1996).

[27] The "lumping" of these two types of investment together introduces a positive, mechanical relationship between regulation and observed investment. A preferred measure of capital stock would exclude the investments in pollution abatement equipment that were mandated by the amendments. The 1986 Pollution Abatement Costs and Expenditures Survey provides some indirect evidence on the magnitude of this bias. It shows that the heaviest-polluting industries devote approximately 4–10 percent of total investment to abatement equipment. This share is likely to be larger in nonattainment counties and indicates that the upward bias may not be insignificant (see Becker 2001).

[28] A book value system permanently records the value of an investment at its purchase price. This value is never updated to reflect inflation or changes in the good's market value. Therefore, the relative contribution of recent investment, which is entered in current dollars, is overstated. A perpetual inventory measure of capital stock accounts for these changes but is not feasible with the Census of Manufactures questionnaire.

TABLE 6
Estimated Regression Models for the Percentage Change in Capital Stock and Shipments

	(1)	(2)	(3)	(4)
	A. Capital Stock (*N* p 1,607,332)			
CO regulation effect (b_4)	ff .047	ff .047	ff .097	ff .092
	(.043)	(.042)	(.043)	(.062)
O_3 regulation effect (b_5)	ff .009	.016	ff .001	ff .041
	(.022)	(.021)	(.021)	(.029)
SO_2 regulation effect (b_6)	ff .024	ff .048	ff .057	ff .063
	(.047)	(.049)	(.055)	(.048)
TSPs regulation effect (b_7)	.026	.042	.010	ff .043
	(.027)	(.025)	(.024)	(.039)
R^2	.074	.109	.155	.462
	B. Shipments (*N* p 1,737,753)			
CO regulation effect (b_4)	ff .058	ff .036	ff .072	ff .146
	(.029)	(.029)	(.029)	(.046)
O_3 regulation effect (b_5)	.022	.048	.019	ff .032
	(.018)	(.018)	(.016)	(.024)
SO_2 regulation effect (b_6)	ff .007	ff .026	ff .027	ff .010
	(.033)	(.030)	(.030)	(.039)
TSPs regulation effect (b_7)	ff .014	ff .002	ff .010	ff .032
	(.019)	(.018)	(.018)	(.034)
R^2	.127	.142	.185	.516
Industry by period fixed effects	yes	yes	yes	yes
Nonattainment by period fixed effects	yes	yes	no	no
County fixed effects	no	yes	no	no
County by period fixed effects	no	no	yes	yes
Plant fixed effects	no	no	no	yes

Note.—See the note to table 5. The mean five-year growth rates of capital stock and shipments are 36.5 percent and 10.0 percent, respectively.

R^2 statistic. But the null that these extra parameters are jointly equal to zero is not rejected at conventional significance levels. As in the employment regressions, this specification indicates that the nonattainment designations have a larger negative impact on growth. In particular, the estimated regulation effects from this specification are ff 0.092 for CO, ff 0.041 for O_3, ff 0.063 for SO_2, and ff 0.043 for TSPs. The loss of the more than 700,000 degrees of freedom causes three of the four standard errors to increase so that the null hypothesis of zero is not rejected for any one of them.

Shipments

Panel B of table 6 reports estimation results for the growth in constant-dollar shipments. The mean five-year growth rate of shipments is 10.0

percent.[29] In columns 1–3, the regulation effect for CO is negative and statistically distinguishable from zero in two of the three specifications. These estimates indicate that CO nonattainment status is associated with a 3.6–7.2 percent decrease in shipments by CO emitters. The regulation effect for O_3 is small and positive, and those for SO_2 and TSPs are small and negative.

As with the employment and capital stock regressions, controlling for plant fixed effects in column 4 causes the estimated negative effects of nonattainment status to have a greater magnitude. In this specification, the estimated regulation effects are ff 0.146 for CO, ff 0.032 for O_3, ff 0.010 for SO_2, and ff 0.032 for TSPs. Again the interpretation of the standard errors is not obvious, but the regulation effect for CO is the only one that is statistically significant at conventional levels. Overall, these results imply that nonattainment status, particularly CO nonattainment status, is associated with a reduction in shipments by polluting manufacturers.

A Comparison of the Estimates across the Dependent Variables

A comparison of the estimates across the three dependent variables within and across specifications provides a crude view into the “black box” of how firms respond to environmental regulations. For example, consider the regulation effects for CO. In the specifications in columns 1–3, they range from ff 0.075 to ff 0.086 for employment, ff 0.047 to ff 0.097 for capital stock, and ff 0.036 to ff 0.072 for shipments. The estimates from the specification in column 4 are ff 0.163, ff 0.092, and ff 0.146, respectively. Within these two divisions of the specifications, the estimates are approximately equivalent across the dependent variables, particularly in the context of the associated standard errors. The same patternisevidentintheeffectsoftheothernonattainmentdesignations, although they are not as large either economically or statistically. Overall, the estimates suggest that the nonattainment designations cause the growth of employment, capital stock, and shipments to decline by roughly equivalent proportions.[30]

B. Is There Heterogeneity in the Regulation Effects across Industries?

This subsection explores whether the regulation effects vary by industry. This is informative for at least two reasons. First, it serves as an internal

[29] Four-digit industry deflators from the Bartelsman and Gray (1994) NBER Productivity Database are used to express the total value of shipments in 1987 dollars.

[30] It would be informative to have plant-level data on pollution emissions. These data would allow for the calculation of the marginal rate of technical substitution between pollution and labor or capital. These measures of the ease of substitution are important policy parameters and are left for future research.

validity check on the results above. If the negative effects are concentrated in a small subset of industries, it may be reasonable to assume that the overall regulation effects are due to an unobserved factor that is unrelated to regulation. As an example, union activism might differ over time and the union activity in a particular industry might be more heavily concentrated in nonattainment counties (e.g., in the Rust Belt). Further, such an unobserved factor could interact with the dramatic reductions in demand experienced by some industries during the periods under consideration; for instance, employment of production workers in primary metal industries (SIC code 33) declined from 1,059,000 in 1967 to only 538,000 in 1987. Second, it provides an opportunity to measure the effects of these regulations across industries. This could be useful in evaluating the claims that particular industries are especially harmed by the CAAAs.

Table 7 presents the industry-specific regulation effects from the estimation of equation (1) for employment. The results for capital stock and shipments are qualitatively similar but are not presented here because of space considerations. The estimated specification includes plant fixed effects, county by period effects, and industry by period effects, as in column 4 of table 5. The regulation effects are allowed to vary across the industries that emit the relevant pollutant, so there are a total of 23 estimated regulation effects. Columns 1–4 report the industry-specific regulation effects and heteroskedastic-consistent standard errors (in parentheses). Each row pertains to an industry so that by reading down a column, one can compare the pollutant-specific regulation effects in each of the relevant industries. The final row lists the χ^2 statistic and associated *p*-value (in parentheses) from tests that the pollutant-specific regulation effects are equal across industries.

A number of points emerge from the table. First, it is apparent that the estimation of industry-specific regulation effects demands a lot from the data. For example, the standard errors are substantially larger than they were in table 5. Notably, the positive estimates tend to be especially poorly determined.

Second, the four χ^2 tests fail to reject the null hypothesis that the pollutant-specific regulation effects are equal across industries. This is certainly related to the imprecision of the estimates, but an “eyeball” test does reveal striking similarities in the parameters within a column (see especially the CO and TSPs effects).

Third, almost all the emitting industries are negatively affected by the nonattainment designations. Only five of the 23 estimated industry-specific regulation effects are greater than zero. Of these five, four occur in industries that emit other pollutants for which the associated regulation effect is negative; thus the overall effect of the CAAAs on these industries may still be negative. I conclude that the estimated regulation

TABLE 7
Do the Employment Regulation Effects Vary by Industry?

Industry Name (SIC Code)	CO Regulation Effects (1)	O_3 Regulation Effects (2)	SO_2 Regulation Effects (3)	TSPs Regulation Effects (4)
Lumber and wood (24)				ff .006
				(.034)
Pulp and paper (2611–31)	ff .080	ff .110	ff .105	.006
	(.077)	(.056)	(.074)	(.064)
Iron and steel (3312–13,	ff .177	ff .104	.038	ff .012
3321–25)	(.061)	(.068)	(.059)	(.050)
Printing (2711–89)		ff .072		
		(.027)		
Organic chemicals		.071		
(2961–69)		(.151)		
Rubber and plastic (30)		ff .093		
		(.046)		
Fabricated metals (34)		ff .013		
		(.026)		
Motor vehicles (371)		ff .026		
		(.057)		
Inorganic chemicals			ff .089	
(2812–19)			(.113)	
Petroleum refining (2911)	ff .133	.172	ff .180	
	(.092)	(.101)	(.109)	
Stone, clay, and glass (32)		ff .072	.039	ff .063
		(.039)	(.062)	(.039)
Nonferrous metals (333–34)	ff .169		ff .063	
	(.163)		(.147)	
x^2 statistic of equality	1.03	11.67	5.82	1.57
	(.79)	(.17)	(.32)	(.67)

NOTE.—See the note to table 5. All the entries are taken from a single regression in which the dependent variable is the change in plant employment between *t* and *t* ff 5 divided by the mean of the *t* and *t* ff 5 levels. The specification includes plant fixed effects, county by period effects, and industry by period effects, as in col. 4 of table 5. The regulation effects are allowed to vary across the industries that emit the relevant pollutant. Cols. 1–4 report the industry-specific regulation effects and heteroskedastic-consistent standard errors (in parentheses). The last row lists the x^2 statistic and associated *p*-value (in parentheses) from tests that the pollutant regulation effects are equal across industries that emit the relevant pollutant.

effects in table 5 do not reflect the experiences of a small subset of emitting industries.

Fourth, the total effect of the regulations is particularly harsh on industries that emit multiple pollutants in counties that are nonattainment for those pollutants. For example, a literal interpretation of the coefficients suggests that pulp and paper plants located in counties that are nonattainment for all four pollutants at the beginning of a period experience an employment decline of almost 29 percent over five years. Similar calculations suggest that employment declines by 14.1 percent in a period at petroleum-refining plants in counties that are nonattainment for CO, O_3, and SO_2.

C. *Robustness Checks*

This paper has used variation in regulation across counties, industries, and time in an effort to estimate the causal effect of regulation on industrial activity. However, as is always the case with a nonexperimental design, there is a form of unobserved heterogeneity that can explain the findings without a causal interpretation. In addition to the efforts presented above, I probed the robustness of the estimates in a number of other ways but found little evidence that undermines the basic conclusions.

Table 8 reports the results of some of these robustness checks in columns 1–3. The entries are the estimated regulation effects and heteroskedastic standard errors (in parentheses). The results for the three dependent variables are in separate panels. Each column represents a different specification or sample. All specifications include county by period fixed effects and industry by period indicators. The results are qualitatively similar when the specification with plant fixed effects is fit, but the standard errors increase substantially because two of the robustness checks significantly cut the sample size. The entries in column 0 are taken from column 3 of tables 5 (employment) and 6 (capital stock and shipments) and should be compared to the entries in the other columns.

One potential source of bias arises from the manner in which nonattainment status is determined and dynamics in the growth of manufacturing activity. Recall that a county's nonattainment designations are determined by its pollution concentrations, which are increasing in manufacturing activity. Thus nonattainment status in the first year of a period is likely an increasing function of previous growth. This may induce a mechanical correlation between the regulation variables and the unobserved components of the dependent variable in equation (1) if manufacturing growth follows a dynamic process. When the process is mean-reverting, this correlation is likely to bias the estimated regulation effects downward.[31]

To determine whether the results above are due to dynamics, column 1 presents results from the estimation of an equation that includes as controls the lagged value of the dependent variable (i.e., the percentage change between *t* ff 5 and *t* ff 10) and interactions of the lag with the four pollutant-emitted indicators. The parameters from the lagged de-

[31] To understand the direction of bias, consider the case in which there is "above-average" growth among emitters of a pollutant in a county in the period between *t* ff 5 and *t* ff 10. This growth might cause the county to be designated nonattainment in *t* ff 5. If the dependent variable follows a mean-reverting process, these polluters are likely to have smaller growth in the period between *t* and *t* ff 5. This slower growth would have occurred even in the absence of regulation, yet the regression would attribute this decline to regulation.

TABLE 8
PROBING the Robustness of the Regulation Effects

	Base Specification (0)	Dynamic Model (1)	Limit Sample to "Stayers" (2)	4.5% Emission Rule (3)
	A. Total Employment			
CO regulation effect (b_4)	ff .086	ff .094	ff .059	ff .097
	(.030)	(.028)	(.023)	(.028)
O_3 regulation effect (b_5)	ff .011	ff .007	ff .019	ff .016
	(.010)	(.010)	(.008)	(.010)
SO_2 regulation effect (b_6)	.003	.005	.010	.006
	(.029)	(.027)	(.021)	(.028)
TSPs regulation effect (b_7)	ff .020	ff .013	ff .022	ff .013
	(.013)	(.014)	(.011)	(.013)
	B. Capital Stock			
CO regulation effect (b_4)	ff .097	ff .134	ff .110	ff .115
	(.043)	(.041)	(.033)	(.040)
O_3 regulation effect (b_5)	ff .001	ff .007	ff .021	ff .009
	(.021)	(.021)	(.016)	(.020)
SO_2 regulation effect (b_6)	ff .057	ff .085	ff .032	ff .006
	(.055)	(.045)	(.036)	(.052)
TSPs regulation effect (b_7)	.010	.002	ff .038	.010
	(.024)	(.024)	(.021)	(.033)
	C. Shipments			
CO regulation effect (b_4)	ff .072	ff .092	ff .048	ff .075
	(.029)	(.027)	(.024)	(.027)
O_3 regulation effect (b_5)	.019	ff .019	.000	.016
	(.016)	(.016)	(.015)	(.016)
SO_2 regulation effect (b_6)	ff .027	ff .054	ff .023	ff .020
	(.030)	(.025)	(.025)	(.030)
TSPs regulation effect (b_7)	ff .010	ff .054	ff .037	.008
	(.018)	(.016)	(.015)	(.020)

NOTE.—See the notes to tables 5 and 6. The entries are the estimated regulation effects and heteroskedastic standard errors (in parentheses) from separate regressions for the three dependent variables. The dependent variable is identified in the panel heading. Each column represents a different specification or sample. All specifications include county by period effects and industry by period effects. The entries in col. 0 are taken from col. 3 of tables 5 (employment) and 6 (capital stock and shipments) and should be compared to the other columns. In col. 1, the lagged dependent variable is included as a regressor, and its effect is allowed to vary by the pollutant emitted. The sample size is 884,812 for employment, 921,403 for capital stock, and 944,596 for shipments. In col. 2, the sample is limited to stayer plants, and the respective sample sizes are 762,513, 764,115, and 768,096. In col. 3, industries that account for at least 4.5 percent of industrial sector emissions of a pollutant are classified as an emitter of that pollutant (see App. table A2).

pendent variables are not reported in the table but provide evidence of dynamic patterns of growth in manufacturing activity. However, the commonality of the estimates in columns 0 and 1 implies that the regulation effects are not due to these dynamics.[32]

It is frequently assumed that environmental regulations primarily affect the location decisions of new plants (e.g., Bartik 1985; McConnell and Schwab 1990) because "grandfather" clauses and political lobbying

[32] The estimates are virtually identical when the col. 0 sample is limited to plants with nonmissing lagged dependent variables.

protect incumbent plants. In column 2 of table 8, the sample is limited to "stayers," that is, plants that are operating in both the first and last years of a period. The estimated regulation effects in this column are remarkably similar to those from the base specification, indicating that the regulations also restrict the growth of stayers. The negative coefficients from the capital stock regression are noteworthy because the regulations frequently require stayers to install "end of the line" pollution abatement equipment that increases measured investment (but not productive investment).[33]

I also examine the sensitivity of the estimated regulation effects to the definitions of emitting status. For example, column 3 of table 8 presents the results from regressions in which the group of emitters is expanded such that industries that account for more than 4.5 percent of the industrial sector's emissions of a pollutant are classified as an emitter of that pollutant (see App. table A2). The estimated regulation effects are generally unchanged by this expansion of the list of emitters. Further, I tested whether the effects differed when an industry is required to account for at least 9 percent of industrial sector emissions to qualify as a polluter. The estimated regulation effects are also qualitatively similar in this case.[34]

Another possible source of bias is that plants located in a county that is currently nonattainment but is "expected" to become attainment in the near future might delay investments until the regulation designation is changed. In the presence of this type of temporal shifting, the estimated regulation effects would be negative; but over longer periods, regulation would have no effect on manufacturing activity. In order to explore this possibility, I restricted the sample so that plant observations from counties that are nonattainment for a particular pollutant in a given period but attainment for the same pollutant in the next period are dropped. This sample restriction is implemented four separate times, once for each of the pollutant-specific nonattainment designations. The estimated regulation effects from these restricted samples are statistically indistinguishable from estimates based on the full sample. Consequently, it is unlikely that this form of temporal shifting of investment is the source of the estimated regulation effects.

A further potential source of bias comes from unobserved regional shocks to industries. I estimated a model that included industry by period by region fixed effects, where industry is defined as one of the 13 industries described above and regions are the nine Census Bureau regions of the United States. The estimated regulation effects from this

[33] Greenstone (1998) provides evidence on the regulations' effect on plant locationand exit decisions.

[34] These results and the other results discussed in the remainder of this subsection are available from the author.

specification are also similar to those presented in tables 5 and 6. Additionally, I fit a model that allows the industry shocks to vary at the state level rather than the census region level. In a further specification, I disaggregated industry and estimated an equation that includes SIC three-digit industry by period by census region fixed effects. Neither of these alternatives changes the estimated regulation effects by a meaningful amount. Overall, there is little evidence that the regulation effects are due to regional industry shocks.

Finally, owing to the coincidence of the implementation of these regulations and the decline in manufacturing activity in "Rust Belt" states, it is sometimes thought that the regulations caused this decline (e.g., Kahn 1999). To examine this possibility, I separately estimated the regulation effects on samples from the Rust Belt and non–Rust Belt states.[35] Across the three measures of manufacturing activity, the estimates indicate that the regulations retard the growth of polluting manufacturers in both sets of states.

VI. The Magnitude of the Regulation Effects and Their Interpretation

The analysis above indicates that the CAAAs reduced the *relative* growth of pollution-intensive manufacturing activity in nonattainment counties. This section provides answers to three important questions about the estimated regulation effects. How large are they? Can they be used to assess claims that the CAAAs cause manufacturers to shift production (and jobs) abroad? Further, do they provide estimates of the costs of the nonattainment designations that can be compared with estimates of their benefits?

A. The Magnitude of the Regulation Effects

Table 9 develops two measures of the magnitude of the regulation effects. Notice that there are three panels, one for each of the measures of manufacturing activity. Column 1 presents the estimated regulation-induced change in the measures of activity. This is calculated by multiplying the sum of the activity in targeted plants (recall table 2) by the relevant estimated regulation effects from the specification that includes plant fixed effects (i.e., col. 4 of tables 5 and 6). The estimated regulation-induced changes are presented separately by pollutant, and their sum is listed in the "all manufacturers" row. Column 2 lists the 95 percent confidence interval of these estimates. Column 3 reports the change in

[35] The Rust Belt is defined to include Illinois, Indiana, Michigan, New York, Ohio, and Pennsylvania.

TABLE 9
Two Measures of the Magnitude of the Regulation Effects

	Estimated Regulation-Induced Change, 1972–77 to 1982–87		Change 1972–77 to 1982–87	Mean of 1972–77 and 1982–87 Levels	Ratio of Col. 1 to Col. 3	Ratio of Col. 1 to Col. 4
	Mean (1)	95% Confidence Interval (2)	(3)	(4)	(5)	(6)
		A. Total Employment				
CO emitters	ff 119,100	[ff 54,600, ff 183,500]	ff 296,502	892,312	.402	ff .133
O_3 emitters	ff 423,400	[ff 169,400, ff 677,400]	ff 169,000	5,496,651	2.505	ff .077
SO_2 emitters	800	[57,400, ff 55,800]	ff 359,821	1,537,994	ff .002	.001
TSPs emitters	ff 50,200	[48,200, ff 148,500]	ff 374,081	1,884,883	.134	ff .027
All manufacturers	ff 591,900	[ff 118,400, ff 1,065,200]	ff 250,183	17,215,016	2.366	ff .034
		B. Capital Stock (Millions of Dollars)				
CO emitters	ff 7,500	[2,400, ff 17,500]	65,977	110,639	ff .114	ff .068
O_3 emitters	ff 18,600	[7,200, ff 44,300]	175,235	258,645	ff .106	ff .072
SO_2 emitters	ff 4,800	[2,400, ff 11,900]	85,092	144,078	ff .056	ff .033
TSPs emitters	ff 5,700	[4,500, ff 15,900]	56,635	108,261	ff .101	ff .053
All manufacturers	ff 36,600	[16,400, ff 89,600]	409,687	565,888	ff .089	ff .065
		C. Shipments (Millions of 1987 Dollars)				
CO emitters	ff 25,700	[ff 9,800, ff 41,500]	ff 25,601	235,616	1.003	ff .109
O_3 emitters	ff 40,500	[19,000, ff 100,000]	2,281	773,443	ff 17.751	ff .052
SO_2 emitters	ff 1,500	[10,000, ff 13,000]	ff 29,806	310,140	.050	ff .005
TSPs emitters	ff 7,600	[8,200, ff 23,500]	ff 24,581	211,875	.310	ff .036
All manufacturers	ff 75,300	[27,400, ff 178,000]	227,673	2,051,492	ff .331	ff .037

Note.—The entries in col. 1 are calculated by multiplying the parameter estimates from col. 4 of tables 5 (employment) and 6 (capital stock and shipments) and the level of the outcomes in emitters in nonattainment counties; table 2 presents the employment levels. For instance, the effect of CO regulation on employment in CO-emitting industries is calculated by multiplying the estimated effect of CO nonattainment (ff .163) by the sum of the levels of employment in CO-emitting plants located in CO nonattainment counties for 1972–77 (201,108), 1977–82 (302,989), and 1982–87 (226,294), which yields an estimated change of ff 119,100 jobs. Col. 2 presents the 95 percent confidence interval of this estimate based on the heteroskedastic-consistent standard errors. The entries in col. 3 are the difference between the 1972–77 and 1982–87 levels of the outcome variables, and the entries in col. 4 are the means of these two values. The shipments measures were converted to 1987 dollars using the Bartelsman and Gray (1994) NBER Productivity Database four-digit deflators.

the measure of activity between the period in which the CAAAs were first in force and the last period (i.e., 1972–77 and 1982–87), separately for emitters of each of the pollutants and the entire manufacturing sector. Finally, column 4 lists the mean of the levels from these two periods for the same categories of plants.

The entries in columns 1, 3, and 4 are used to calculate the two measures of the magnitude of the regulation effects. Column 5 reports the ratio of the entries in columns 1 and 3, and column 6 lists the ratio of columns 1 and 4. Thus these columns normalize the regulation-induced changes by the total change in and mean of the measures of activity, respectively.

Panel A reports these calculations for employment. For example, they indicate that employment in CO-emitting industries located in CO nonattainment counties declined by 119,100 jobs (relative to CO emitters in CO attainment counties) in the first 15 years in which the CAAAs were in force.[36] The 95 percent confidence interval of this estimate is [ff 54,600, ff 183,500]. Analogous calculations indicate that the cumulative regulation-induced change (95 percent confidence interval) in employment in nonattainment counties is ff 423,400 [ff 169,400, ff 677,400] for O_3, 800 [57,400, ff 55,800] for SO_2, and ff 50,200 [48,200, ff 148,500] for TSPs. The large decline in O_3 employment reflects the high levels of employment in O_3-emitting industries. The sum of the regulation-induced changes is ff 591,900 [ff 118,400, ff 1,065,200].

Column 5 reports that the total regulation-induced change in employment is almost 2.4 times as large as the decline in manufacturing sector employment (roughly 250,000 jobs). This ratio is large, but manufacturing sector employment was essentially flat in these periods. The second measure reveals that the regulation-induced change in employment in nonattainment counties was a more modest 3.4 percent of total manufacturing sector employment.

Panels B and C present the analogous calculations for capital stock and shipments, respectively. The cumulative regulation-induced changes in capital stock and shipments across all four regulations are $36.6 billion [$16.4 billion, ff $89.6 billion] and $75.3 billion (1987 dollars) [$27.4 billion, ff $178.0 billion], respectively. These changes are 8.9 percent and 33.1 percent of the total change in these measures of manufacturing activity. When they are normalized by the mean levels of capital stock and shipments, they are 6.5 percent and 3.7 percent, respectively.

Overall, these two measures indicate that during the first 15 years in which the CAAAs were in force, the cumulative regulation-induced changes in manufacturing activity in nonattainment counties were not insignificant relative to either changes in or the level of total manufacturing sector activity. It is important to bear in mind, however, that the legislation also specified regulations for attainment counties. Consequently, it is likely that the total effect of the CAAAs is even larger than indicated in table 9.

B. Interpretation

It would be informative if the estimated regulation effects could be used to determine how much production (and employment) was shifted abroad as a result of the nonattainment designations.[37] This would pro-

[36] This is calculated by multiplying the estimated effect of CO nonattainment status (ff 0.163) by the sum of the levels of employment in CO-emitting plants located in nonattainment counties for 1972–77 (201,108), 1977–82 (302,989), and 1982–87 (226,294).

[37] A related question is whether environmental regulations alter the international location decisions of polluters. An extrapolation of this paper's findings to this question suggests that international differences in the stringency of environmental regulation will tend to shift polluters' production to countries with relatively lax environmental standards.

vide one measure of the national costs of these regulations. Unfortunately, such a calculation is not possible because it cannot be determined whether the lost activity in nonattainment counties moved to foreign countries or attainment counties. Since it is likely that the regulation effects partially reflect some shifting of manufacturing activity within the United States, they probably *overstate* the national loss of activity due to the nonattainment designations. Moreover, the possibility of intracountry shifting means that the regulation effects are also likely to overstate losses in nonattainment counties. The reason is that the identification strategy relies on comparisons between nonattainment and attainment counties, which leads to "double counting" when production is moved from a nonattainment county to an attainment one.[38]

There are at least two reasons to doubt that the regulation effects entirely reflect a movement of plants from nonattainment to attainment counties. First, counties frequently move into and out of nonattainment status. Thus firms may consider it unlikely that they can remain in the United States and escape future regulation. Second, production in many of the regulated industries (e.g., iron and steel and pulp and paper) requires substantial "sunk" costs that make it costly to shift locations.

The estimated regulation effects have an additional limitation as a measure of the costs of regulation. They are calculated in terms of employment, investment, and shipments, but these measures are not readily comparable to standard measures of the benefits of regulation. The conversion of these measures into a monetary unit would have great practical importance. For instance, it would then be possible to compare the costs of the regulations with hedonic housing market estimates of the monetary gains to homeowners from regulation-induced pollution reductions.

A full monetizing of the regulation-induced losses is left to future research, but it is worth noting that this task is tractable. In a freely functioning market economy, jobs and capital are not lost or made obsolete. In response to a shock such as the imposition of environmental regulations, these factors of production generally become employed in another capacity. Thus the losses due to regulation are the adjustment costs associated with the shifting of resources to new sectors. It is evident that monetized estimates of the costs of the CAAAs require reliable estimates of the magnitude of these frictions.

Recent research indicates that these frictions may be quite substantial and can persist for as long as a decade (Blanchard and Katz 1992). Jacobson, LaLonde, and Sullivan (1993) document that displaced work-

[38] In the extreme, the estimated regulation effects entirely reflect a movement of manufacturing activity from nonattainment to attainment counties. In this scenario there is no loss of production (and jobs) to foreign countries, and the regulation effects overstate the lost production in nonattainment counties by a factor of two.

ers endure substantial wage losses. Further, Goolsbee and Gross (2000) and Ramey and Shapiro (2001) show that it is costly for firms to adjust their capital stock in response to demand shocks. Consequently, workers and firms that were affected by the CAAAs may have suffered substantial losses.

VII. Conclusions

This paper provides new evidence that environmental regulations restrict industrial activity. I find that in the first 15 years after the CAAAs became law (1972–87), nonattainment counties (relative to attainment ones) lost approximately 590,000 jobs, $37 billion in capital stock, and $75 billion (1987 dollars) of output in polluting industries. Although these estimates are not derived from a randomized experiment and therefore cannot meet a strict definition of causality, they provide robust evidence that these regulations deter the growth of polluters. In the first place, the findings are derived from the most comprehensive data available on clean air regulations and manufacturing activity. Second, the preferred statistical model for plant-level growth controls for all permanent plant characteristics, unrestricted industry shocks, and unrestricted county shocks. Third, the effects are robust across a variety of specifications. Finally, the regulation effects are evident across three different measures of manufacturing activity and a wide range of polluting industries.

The federal standards for ozone and particulates were tightened recently, causing a substantial increase in the number of nonattainment counties.[39] The balance of evidence from this paper suggests that the new nonattainment counties will experience reductions in employment, investment, and shipments in polluting industries. To gain a clearer understanding of whether it is worthwhile to incur the costs associated with these reductions, it is crucial to understand the regulations' effectiveness at cleaning the air and the benefits of cleaner air. Recent research finds that these policies are effective at reducing concentrations of air pollution and that cleaner air, particularly reductions in TSPs, provides substantial monetary benefits to homeowners and reduced infant mortality rates (Smith and Huang 1995; Henderson 1996; Chay and Greenstone 2000, 2002*a*, 2002*b*). Regardless of whether these policies pass or fail a cost-benefit test, this paper's findings undermine the contention that environmental regulations are costless or even beneficial for the regulated.

[39] Although legal wrangling over this policy change is not concluded, the Supreme Court's *Whitman v. American Trucking Associations* decision appears to uphold the EPA's decision to tighten these standards.

TABLE A1
SELECTED National Ambient Air Quality Standards

	Maximum Allowable Concentration (Primary Standard)
Carbon monoxide:	
Maximum 8-hour concentration	9 parts per million
Maximum 1-hour concentration	35 parts per million
Nitrogen dioxide:	
Annual arithmetic mean	.053 parts per million
Ozone:	
Maximum 1-hour concentration	.12 parts per million (after 1979) .08 parts per million (through 1979)
Sulfur dioxide:	
Annual arithmetic mean	.03 parts per million
Maximum 24-hour concentration	.14 parts per million
Total suspended particulates:	
Annual geometric mean	75 micrograms per cubic meter
Maximum 24-hour concentration	260 micrograms per cubic meter

NOTE.—A county is in violation of one of the hourly based standards (i.e., one-hour, eight-hour, or 24-hour) if it exceeds the standard more than once in a year. In 1987 the EPA switched its focus from the regulation of all particulates (i.e., TSPs) to small particulates (i.e., PM10s). In 1997 the ozone standard was revised, and the particulates standard was further modified to regulate even smaller particulates (i.e., PM2.5s).

Data Appendix

A. Determining the County-Level, Pollutant-Specific Regulation Designations

The centerpiece of the Clean Air Act Amendments is the annual county-level assignment of nonattainment and attainment status for CO, O_3, SO_2, and TSPs. The legislation specifies that the pollutant-specific designations be based on whether a county's ambient pollution concentration exceeds the relevant federal air quality standard. Table A1 lists the standards. This section describes how these designations are determined for each of the four periods (i.e., 1967–72, 1972–77, 1977–82, and 1982–87) examined in this paper.

Although the 1970 amendment passed before the 1967–72 period ended, the associated enforcement activities did not commence until late 1972 (Liroff 1986). Consequently, every county is designated attainment for all four pollutants in the 1967–72 period.

The determination of the nonattainment designations in the 1977–82 and 1982–87 periods is relatively straightforward. In 1978 the EPA began to publish annually a list of nonattainment counties in the *Code of Federal Regulations*.[40] For each of the regulated pollutants, the CFR lists every county as "does not meet primary standards," "does not meet secondary standards," "cannot be classified," "better than national standards," or "cannot be classified or better than national standards." Further, the CFR occasionally indicates that a part of a county did not meet the primary standards. For the 1977–82 (1982–87) period, a county is assigned to the pollutant-specific nonattainment category if all or part of it failed to meet the pollutant-specific "primary standards" in 1978 (1982); otherwise, it is assigned to the pollutant-specific attainment category. These annual county-level, pollutant-specific designations were hand entered for the 3,070 U.S. counties.

[40] Vernon Henderson and Randy Becker generously allowed me to photocopy the relevant sections of the CFR.

The determination of the identities of the nonattainment counties in the 1972–77 period is more complicated. The EPA did not publish them in the early years of regulation, and I was told that records from that period "no longer exist." Consequently, I filed a Freedom of Information Act request and obtained the EPA's "Quick Look Report" data file, which contains annual summary information on the readings from each EPA pollution monitor.[41] This file is used to replicate the EPA's statutory selection rule; counties with monitor readings exceeding the pollutant-specific national standard in 1972 are assigned to the pollutant-specific nonattainment category for the 1972–77 period. All other county by pollutant combinations are designated attainment.[42]

B. Determining Which Plants Were Subject to the Regulations

An important part of the analysis is the determination of which manufacturing plants (or industries) were not targeted by the regulations in the examined period. A historical list of regulated plants or industries is unavailable from the EPA. Consequently, I devised a system to divide the manufacturing sector into emitters and nonemitters that attempts to mimic the EPA's focus on the dirtiest plants and industries in the initial years of regulation.

The EPA's estimates of industrial emissions are used to determine the pollutants emitted by each industry. These estimates are reproduced in table A2. The table lists the estimated annual emissions of each of the regulated pollutants by industry, as well as each industry's share of total industrial sector emissions. Industries that are excluded from the table either produce negligible levels of the regulated pollutants or had escaped the EPA's attention as late as the early 1990s. Communications with EPA officials indicate that it is unlikely that the excluded industries were subject to significant regulatory oversight in the 1970s and 1980s.

In the assignment of polluter status to industries, one possibility is to assume that the industries listed in table A2 are regulated for all the pollutants. Since some industries are major polluters of a particular pollutant but not of another, it is evident that this is not a sensible approach. Consequently, I label all industries that account for at least 7 percent of the industrial sector emissions of a pollutant to be an emitter of that pollutant; excluded industries and those whose emissions fall below the 7 percent threshold are considered nonemitters of that pollutant. An industry is designated an O_3 emitter if it exceeds the 7 percent threshold for either nitrogen dioxide or volatile organic compounds, both of which are precursors of ozone. The results are insensitive to other "reasonable" definitions of emitter status. These results are discussed in Section V.

[41] This date file comes from the EPA's Air Quality Subsystem database and contains annual statistics on the readings from all state and national pollution monitors for the four criteria pollutants.

[42] I tested whether the results were sensitive to the choice of a pollution monitor–based definition of which county/pollutant combinations were heavily regulated for this period (i.e., 1972–77). The paper's conclusions are insensitive to dropping the 1972–77 period from the sample.

TABLE A2

ANNUAL Industrial Sector Pollutant Releases by Industry

	CARBON Monoxide		NITROGEN Dioxide		VOLATILE Organic Compounds		Sulfur Dioxide		TOTAL Suspended Particulates		
INDUSTRY (SIC Code)	Emissions (1)	Share (2)	Emissions (1)	Share (2)	Emissions (1)	Share (2)	Emissions (1)	Share (2)	Emissions (1)	Share (2)	EMITTER Status (3)
Metal mining (10)	5,391	.2%	28,583	1.6%	1,283	.1%	84,222	3.5%	140,052	15.4%	*
Nonmetal mining (14)	4,525	.1%	28,804	1.6%	1,736	.1%	24,129	1.0%	167,948	18.5%	*
Lumber and wood products (24)	123,756	3.5%	42,658	2.4%	41,423	3.0%	9,149	.4%	63,761	7.0%	TSPs
Wood furniture and fixtures (parts of 25)†	2,069	.1%	2,981	.2%	59,426	4.4%	1,606	.1%	3,178	.3%	Clean
Pulp and paper (2611–31)	624,291	17.5%	394,448	21.7%	96,875	7.1%	341,002	14.0%	113,571	12.5%	CO / O_3 / SO_2 / TSPs
Printing (2711–89)	8,463	.2%	4,915	.3%	101,537	7.5%	1,728	.1%	1,031	.1%	O_3
Inorganic chemicals (2812–19)	166,147	4.7%	108,575	6.0%	52,091	3.8%	182,189	7.5%	39,082	4.3%	SO_2
Organic chemicals (2861–69)	146,947	4.1%	236,826	13.0%	201,888	14.8%	132,459	5.4%	44,860	4.9%	O_3
Petroleum refining (2911)	419,311	11.8%	380,641	21.0%	309,058	22.7%	648,153	26.6%	36,877	4.1%	CO / O_3 / SO_2
Rubber and miscellaneous plastic products (30)	2,090	.1%	11,914	.7%	140,741	10.3%	29,364	1.2%	5,355	.6%	O_3
Stone, clay, glass, and concrete (32)	58,043	1.6%	338,482	18.6%	30,262	2.2%	339,216	13.9%	171,853	18.9%	O_3 / SO_2 / TSPs
Iron and steel (3312–33, 3321–25)	1,518,642	42.6%	138,985	7.7%	82,292	6.0%	238,268	9.8%	83,017	9.1%	CO / O_3 / SO_2 / TSPs
Nonferrous metals (333–34)	448,758	12.6%	55,658	3.1%	27,375	2.0%	373,007	15.3%	22,490	2.5%	CO / SO_2
Fabricated metals (34)	3,851	.1%	16,424	.9%	102,186	7.5%	4,019	.2%	3,136	.3%	O_3
Electronics (36)	367	.0%	1,129	.1%	4,854	.4%	453	.0%	293	.0%	Clean
Motor vehicles, bodies, and parts (371)	35,303	1.0%	23,725	1.3%	101,275	7.4%	25,462	1.0%	12,853	1.4%	O_3
Dry cleaning (721)	101	.0%	179	.0%	7,310	.5%	152	.0%	28	.0%	*
Industrial sector total	3,568,055		1,814,927		1,361,612		2,434,578		909,385		

SOURCE.—EPA Sector Notebook Project (1995).

Note.—For each pollutant, emissions in col. 1 lists the number of short tons emitted per year. Share in col. 2 reports the fraction of industrial sector emissions. The paper's analysis designates an industry an emitter of a pollutant if it accounts for at least 7 percent of industrial sector emissions. Each industry's emitter status is summarized in col. 3. Nitrogen dioxide and volatile organic compounds are the primary ingredients of ozone (O_3). If an industry emitted more than 7 percent of either of these pollutants, it is designated an O_3 emitter. The remainder of the manufacturing sector is designated nonemitters of all criteria pollutants and labeled clean.

* Metal mining, nonmetal mining, and dry cleaning are outside of the manufacturing sector.

† Wood furniture and fixtures comprises the following SIC codes: 2511, 2512, 2517, 2519, 2521, 2531, and 2541.

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Do local energy prices and regulation affect the geographic concentration of employment?☆

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Manufacturing industries differ with respect to their energy intensity, labor-to-capital ratio and their pollution intensity. Across the United States, there is significant variation in electricity prices and labor and environmental regulation. This paper examines whether the basic logic of comparative advantage can explain the geographical clustering of U.S. manufacturing. We document that energy-intensive industries concentrate in low electricity price counties and labor-intensive industries avoid pro-union counties. We find mixed evidence that pollution-intensive industries locate in counties featuring relatively lax Clean Air Act regulation.

1. Introduction

Between 1998 and 2009, aggregate U.S. manufacturing jobs declined by 35 percent while the total production of this industry grew by 21 percent.[1] This loss of manufacturing jobs has important implications for the quality of life of the middle class. Manufacturing offers less educated workers employment in relatively well paying jobs (Neal, 1995). Despite public concerns about the international outsourcing of jobs, over eleven million people continue to work in the U.S. manufacturing sector.[2] The ability of local areas to attract and retain such manufacturing jobs continues to play an important role in determining the vibrancy of their local economy (Greenstone et al., 2010).

Ongoing research examines the role that government regulations and local factor prices play in attracting or deflecting manufacturing employment. During a time when unemployment rates differ greatly across states, there remains an open question concerning the role that regulation plays in determining the geography of productive activity. A leading example of this research is Holmes' (1998) study that exploited sharp changes in labor regulation at adjacent state boundaries. He posited that counties that are located in Right-to-Work states have a more "pro-business" environment than their nearby neighboring county located in a pro-union state. He used this border-pairs approach to establish that between 1952 and 1988 there has been an increasing concentration of manufacturing activity on the Right-to-Work side of the border. A recent Wall Street Journal piece claimed that, between the years 2000 and 2008, 4.8 million Americans moved from union states to Right-to-Work states.[3]

In this paper, we build on Holmes' core research methodology along three dimensions. First, we focus on the modern period from 1998 to 2009. During this time period, the manufacturing sector experienced significant job destruction as intense international competition has taken place (Davis et al., 2006; Bernard et al., 2006). This time period covers the start of the recent deep downturn in the national economy and the earlier 2000 to 2001 recession. Past research has documented that industrial concentration is affected by energy prices

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[1] The U.S. Bureau of Labor Statistics reports employment by sector. From 1998 to 2009, manufacturing employment fell from 17.6 million to 11.5 million (http://data.bls.gov/timeseries/CES3000000001?data_tool = XGtable). The United Nations Statistics division reports gross value added by kind of economic activity at constant (2005) US dollars. From 1998 to 2009, manufacturing value went from $1348 billion to $1626 billion (http://data.un.org/Data.aspx?d = SNAAMA&f = grID%3a202%3bcurrID%3aUSD%3bpcFlag%3a0%3bitID%3a12).

[2] In March, 2011, 11.67 million people worked in manufacturing (NAICS 31–33) (source: http://www.bls.gov/iag/tgs/iag31-33.htm).

[3] Arthur B. Laffer and Stephen Moore. "Boeing and the Union Berlin Wall." http://online.wsj.com/article/SB10001424052748703730804576317140858893466.html

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(Carlton, 1983), environmental regulation (Becker and Henderson, 2000; Greenstone, 2002; Walker, 2012), and labor regulation and general state level pro-business policies (Holmes, 1998; Chirinko and Wilson, 2008). Second, we use the border-pair methodology to study the relative importance of these three key determinants of the geographic concentration of manufacturing jobs in one unified framework. Third, we examine the heterogeneity of industries' response to these policies.

We estimate a reduced form econometric model of equilibrium employment variation across counties that allow us to study how energy regulation, labor regulation and environmental regulation are associated with the spatial distribution of employment while holding constant the other policies of interest. Our identification strategy exploits within border-pair variation in energy prices and regulation to tease out the role that each of these factors play in influencing the geographical patterns of manufacturing employment. As we discuss below, county border pairs share many common attributes including local labor market conditions, spatial amenities, and proximity to markets. We compare our estimates of policy effects in regression results with different levels of geographic controls to see how robust our results are across different specifications.

This paper studies where different industries cluster across different types of counties as a function of county regulation status. In the case of manufacturing, we disaggregate manufacturing into 21 three-digit NAICS industries. These industries differ along three dimensions; the industry's energy consumption per unit of output, the industry's labor-to-capital ratio, and the industry's pollution intensity. We model each county as embodying three key bundled attributes; its utility's average industrial electricity price, its state's labor regulation, and the county's Clean Air Act regulatory status.

The basic logic of comparative advantage yields several testable hypotheses. In a similar spirit as Ellison and Glaeser (1999), we test for the role of geographical "natural advantages" by studying the sorting patterns of diverse industries. Energy-intensive industries should avoid high electricity price counties.[4] Labor-intensive manufacturing should avoid pro-union counties. Pollution-intensive industries should avoid counties that face strict Clean Air Act regulation. We use a county-industry level panel data set covering the years 1998 to 2009 to test all three of these claims.

The paper also examines the relationship between energy prices and employment for specific industries. We recognize that manufacturing is just one sector of the economy and thus we examine how other major non-manufacturing industries are affected by energy, labor and environmental regulation. For 21 manufacturing industries and 15 major non-manufacturing industries, we estimate this relationship. We find that energy prices are not an important correlate of geographical concentration for most non-manufacturing industries. However, employment in expanding industries such as Credit Intermediation (NAICS 522), Professional, Scientific and Technical Services (NAICS 541), and Management of Companies and Enterprises (NAICS 551) is responsive to electricity prices with implied elasticities of approximately −.15. In comparison, the most electricity-intensive manufacturing industry, primary metals, has an elasticity of −1.17.

2. Empirical framework

Our empirical work will focus on examining the correlates of the geographic clusters of employment and establishments by industry starting in 1998. Building on Holmes' (1998) approach, we rely heavily on estimating statistical models that include border-pair fixed effects. A border pair will consist of two adjacent counties.

Comparing the geographic concentration of employment within a border pair controls for many relevant cost factors. Manufacturing firms face several tradeoffs in choosing where to locate, how much to produce, and which inputs to use. To reduce their cost of production, they would like to locate in areas featuring cheap land, low quality-adjusted wages, lax regulatory requirements and cheap energy. They would also like to be close to final consumers and input suppliers in order to conserve on transportation costs. Within a border pair, we posit that local wages are roughly constant as are location specific amenities and proximity to input suppliers and final consumers.

Our unit of analysis will be a county/industry/year. First we study the geographic concentration of 21 manufacturing industries using the U.S. County Business Patterns (CBP) data over the years 1998 to 2009.[5] The CBP reports for each county and year the employment count, establishment count and establishment count by employment size. This last set of variables is important because the CBP suppresses the actual employment count and reports a "0" for many observations (Isserman and Westervelt, 2006).[6]

Throughout this paper, we assume that each industry differs with respect to its production process (and hence in their firms' response to electricity prices and regulation) but any two firms within the same industry have the same production function. In general, energy inputs and the firm's environmental control technology may be either substitutes or complements with labor in a given industry (Berman and Bui, 2001). Our paper studies the effects of regulations on overall employment, combining both these substitution effects as well as scale effects.

Our main econometric model is presented in Eq. (1). Estimates of Eq. (1) generate new finding about the equilibrium statistical relationship between regulation, electricity prices and manufacturing location choices between 1998 and 2009. The unit of analysis is by county i, county-pair j, industry k, and year t. County i is located in utility u and state s. In most of the specifications we report below, we will focus on counties that are located in metropolitan areas.[7]

$$\begin{aligned} emp_{ijuskt} = {} & \beta_1 P_{ut}^{elec} + \beta_2 P_{ut}^{elec} \times ElecIndex_{kt} + \beta_3 Right_s \times LabCapRatio_{kt} \\ & + \beta_4 Nonattain_{it} + \beta_5 Nonattain_{it} \times PollIndex_k + \beta_6 NoMonitor_i \\ & + \beta_7 NoMonitor_i \times PollIndex_k + \theta_1 ElecIndex_{kt} + \theta_2 Right_s + \theta_3 LabCapRatio_{kt} \\ & + \theta_4 PollIndex_k + f(Poll_{it}) + \delta Z_i + \alpha_j + \gamma_{kt} + \pi_{st} + \varepsilon_{ijuskt}. \end{aligned} \tag{1}$$

In this regression, the dependent variable will be a measure of county/industry/year employment. The first term on the right side of Eq. (1) presents the log of the average electricity prices that the industry faces in a specific county. The second term allows this price effect to vary with the industry's electricity-intensity index. In the regressions, the electricity-intensity index is normalized to range from 0 to 1 for ease in interpreting the results.[8] Third is an interaction term between whether state s has Right-to-Work laws (Right) and the

[4] Energy-intensive industries will also attempt to avoid high oil, coal, and natural gas prices, as well. However, our identification strategy examines differences between neighboring counties and while there are regional differences in coal and natural gas, these differences are likely to be small between neighboring counties.

[5] County Business Patterns (http://www.census.gov/econ/cbp/download/index.htm). We use 1998 as our start date because this was the first year in which NAICS rather than SIC codes where used. All data use the 2002 NAICS definitions.

[6] The CBP suppress employment counts to protect firms' privacy in certain cases. In 35 percent of our observations, employment equals zero despite there being a positive count of establishments in that county, industry and year. To address this issue, we impute the employment data using the establishment count data when suppression occurs. The CBP provides the counts of establishments by firm size category. We take the midpoint of employment for each of these categories and use the county/industry/year establishment count data across the employment size categories (1–4, 5–9, 10–19, 20–49, 50–99, 100–249, 250–499, 500–999, 1000–1499, 1500–2499, 2500–4999 and 5000+) to impute the employment count for observations that are suppressed. We top code the 5000+ employment observations at 6000.

[7] MSA counties account for most of the population (78% of the 1995 US population), manufacturing establishments (78% in sample), and manufacturing workforce (74% in sample).

[8] The NBER productivity data report electricity intensity in electricity usage (in kWh) per dollar value of shipments. We normalize this measure to range from zero to one to simplify the interpretation of the price coefficients.

industry's labor-to-capital ratio (LabCapRatio). Finally, we examine the effect of environmental policy. This includes the interaction of an indicator of nonattainment status (Nonattainment) and a continuous index of pollution from an industry (PollIndex). We also examine the interaction effect of an indicator of whether a county does not monitor the pollutant of interest (NoMonitor) and the PollIndex variable.

In estimating these policy-relevant variables, we try to control for potentially confounding factors. There are several variables that we would estimate in a traditional difference-in-differences model, including the direct effects of ElecIndex, Right, LabCapRatio, and PollIndex: $\theta_1 - \theta_4$. However, all of these are perfectly collinear with the various fixed effects that we estimate. For example, the direct effect of Right-to-Work states cannot be separately identified given the inclusion of state-year fixed effects. We do control for a flexible function of pollution concentration levels, $poll_{it}$.[9] The Z vector has county variables: a county's population in 1970, its distance to the nearest metropolitan area's Central Business District (CBD), the county's land area, and the log of the 1990 housing values.[10] In the core specifications we control for a county-pair fixed effect, industry-year fixed effects and state-year fixed effects. We rely heavily on these border-pair fixed effects to soak up spatial variation in local labor market conditions, climate amenities, and proximity to intermediate input providers and final customers. Past studies such as Dumais et al. (2002) have emphasized the importance of labor pooling as an explanation for why firms in the same industry locate close together. The industry-year fixed effects control for any macro level changes in demand due to shifting national consumption trends or world trade.[11] The state–year fixed effects control for local labor market conditions such as local wage trends and any state policy that affects a firm's propensity to locate within a state. For example, some states such as Missouri have low taxes while others such as California do not.[12]

We use several different dependent variables. We begin by examining the number of manufacturing employees. We also present results that focus on an industry's percentage of total county employment. In another specification, we report results for the natural log of employment, which is estimated only for observations with positive employment. As discussed below, 14 percent of our observations have no establishments and thus no employees.

For each manufacturing industry, we can measure the electricity intensity and the labor–capital ratio. These data are from NBER Productivity Data Base and cover 1997 to 2009.[13] Below, we will also present results for non-manufacturing industries but we cannot measure their electricity, labor, or pollution intensity. As such, our main results focus on manufacturing where we can test for the role of geographic regulations in attracting employment activity.

The interaction terms presented in Eq. (1) allow us to test three hypotheses. The first hypothesis is that energy-intensive industries cluster on the low electricity price side of the border. The second hypothesis is that labor-intensive industries cluster on the Right-to-Work Side of the border. The third hypothesis is that high emission industries cluster in the low environmental regulation side of the border.

We estimate Eq. (1) using weighted least squares. We will also present results in which we instrument for local electricity prices to test whether these prices are driven by exogenous factors. Note that each county/industry/year observation enters multiple times since a county can be adjacent to several counties. We place equal weight on each county/industry/year observation with weights based on a county's number of borders.[14] Multiple entries also require standard error corrections: we need to cluster at this level or one that is more aggregated. We cluster by major utility to allow for serial correlation and spatial correlation.

In a second set of econometric results, we employ a more conventional model without border pairs. We include county fixed effects and exploit within county variation in environmental regulation and electricity prices to estimate the association between these variables and employment clusters. In Eq. (2), the unit of analysis is by county i, industry k, electric utility u, and year t. We estimate Eq. (2) with county, industry–year, and state–year, fixed effects:

$$\begin{aligned}emp_{iuskt} = {} & \beta_1 P^{elec}_{ut} + \beta_2 P^{elec}_{ut} \times ElecIndex_{kt} + \beta_3 Right_s \times LabCapRatio_{kt} \\ & + \beta_4 Nonattain_{it} + \beta_5 Nonattain_{it} \times PollIndex_k + \beta_6 NoMonitor_i \\ & + \beta_7 NoMonitor_i \times PollIndex_k + f(Poll_{it}) + \alpha_i + \gamma_{kt} + \pi_{st} + \varepsilon_{iuskt}.\end{aligned} \quad (2)$$

By exploiting within-county variation over time in electricity prices and environmental regulation, these estimates can be thought of as a short-term response to changes in the relevant explanatory variables. The county fixed effects regression presented in Eq. (2) also addresses the criticism that there are fixed county attributes that are not captured by our controls that could be correlated with the key explanatory variables. If these unobservables are time invariant, then including county fixed effects address this concern.

3. Three margins affecting geographic concentration of employment

A key identifying assumption in this paper is that there exists within county border pair variation in labor regulation intensity, electricity prices, and Clean Air Act intensity that allows us to observe "exogenous" variation.

3.1. Electricity prices

Electricity prices vary across electric utility jurisdictions (see Fig. 1 for county average prices in 1998). Adjacent counties can lie within different electric utility jurisdictions. Each of the approximately 460 U.S. electric utilities charges different electricity prices. In the ideal research design that relies on county-level employment data, each county would be served by one utility. In this case, we would have a sharp spatial regression discontinuity at each county border but this is not the case. Some major counties have multiple utilities. While other utilities span several counties. If two adjacent counties lie within

[9] Counties are more likely to be assigned to nonattainment status if their ambient air pollution levels in the recent past have been higher. If booming counties have high regulation levels, then a researcher could conclude that regulation raises employment levels when in fact reverse causality is generating this relationship. To sidestep this problem, we include a flexible function of the county's ambient pollution level.

[10] Adjacent counties are unlikely to be "twins." The classic monocentric model of urban economics predicts that counties closer to a major Central Business District will feature higher population densities and higher land prices than more suburban counties. We have also estimated specifications that included other county attributes such as a dummy indicating whether the county is the metropolitan area's center county and another dummy that indicates whether the county is adjacent to an Ocean or a Great Lake. The results are robust to controlling for these variables and are available on request. In Appendix Table A1, we present formal tests of whether our explanatory variables included in the Z vector are "balanced." We find that these covariates vary by treatment for high electricity prices, labor regulation, and environmental regulation. In a regression reported in Table 5, we include linear trends for each covariate to test whether our results are robust.

[11] Linn (2009) documents that linkages between manufacturing industries amplify the effect of macro energy price shocks. Given that energy-intensive industries are important input suppliers to other industries, there could be industry–year effects driven by such linkages. Including the industry–year fixed effects helps to address this issue. For more on the macroeconomics impacts of energy price changes see Killian (2008).

[12] Recent empirical work has documented that minimum wage differences across states do not influence the locational choices of low skill jobs (Dube et al., 2010).

[13] See http://www.nber.org/data/nbprod2005.html. We thank Wayne Gray for providing us with data that extends the sample through 2009.

[14] The analytic weights are the inverse of the number of times a given county/industry/year enters the sample.

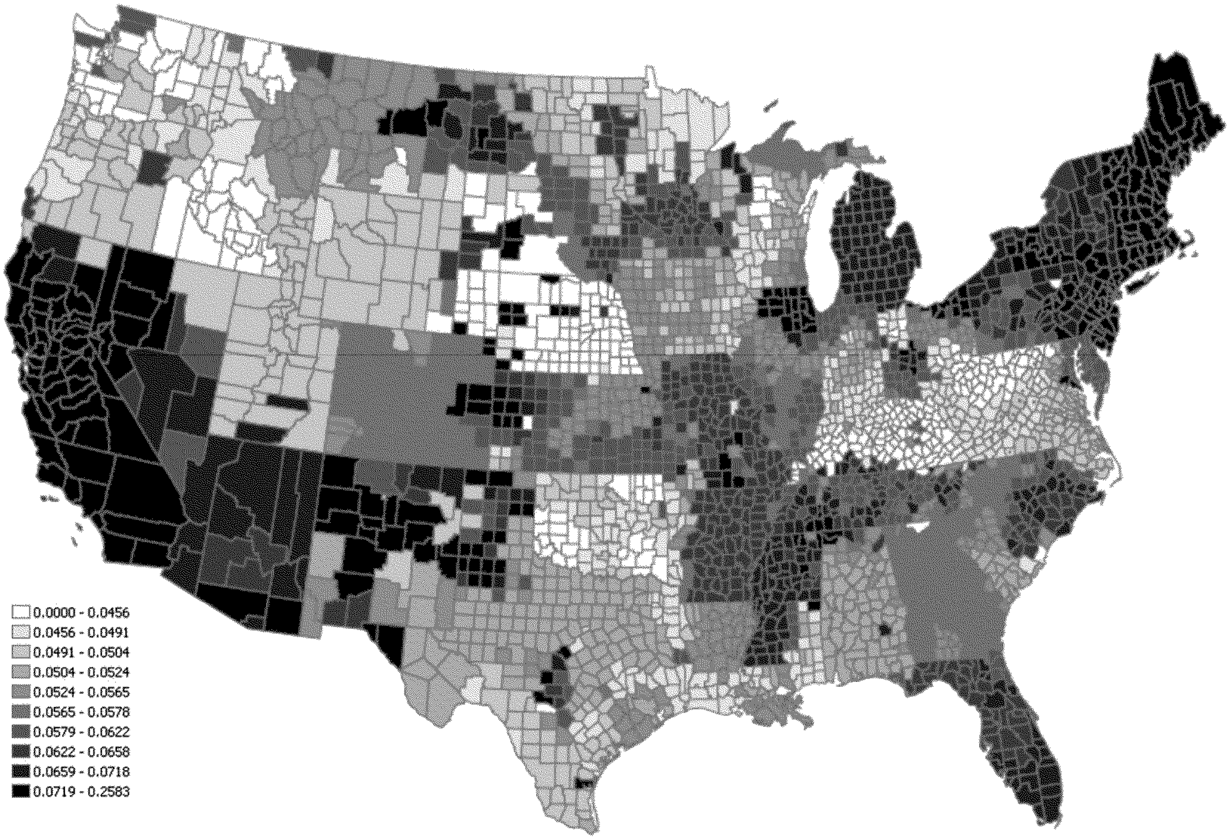

Fig. 1. Industrial electricity prices in 1998 ($/kWh).

the same electric utility district, then there will be no within border pair variation for these counties.[15]

Most of our border pairs are within the same utility area. However, for those pairs that cross utilities, the price differences can be significant. The median price differential is about one cent for border pair counties that lie in different utility areas. For five percent of these counties, the difference is over nine cents a kWh. For firms in electricity-intensive industries, this differential represents about seven percent of revenue. This fact highlights that there are significant cost savings for a subset of industries for choosing to locate in the lower electricity price county within a county-pair.

Most U.S. retail electricity prices are determined through rate hearings where regulated firms can recover rates through average cost pricing. During the early part of our sample, most rates were the function of past costs that had little to do with current production costs.[16] In regions that restructured their wholesale electricity markets, retail rates were frozen for an initial period when utilities were to recover "stranded" assets. Today, the retail prices in these markets reflect wholesale costs, as passed on to consumers through retail competition.

Our electricity price data are constructed from data available from the Energy Information Administration (EIA) form 861.[17] We determine prices by aggregating revenue from industrial customers at any utility that serves these customers in a given county and year. We divide this industrial revenue by the quantity of electricity sold to industrial customers by those utilities in that year.[18] For clustering, we assign the county to one of the 178 major utilities in our sample.[19]

3.2. Labor regulation

We follow Holmes (1998) and assign each county to whether it is located in a Right-to-Work state or not. Today, there are 22 states that are Right-to-Work states. A Right-to-Work law secures the right of employees to decide for themselves whether or not to join or financially support a union. The set of states includes Alabama, Arizona, Arkansas, Florida, Georgia, Idaho, Iowa, Kansas, Louisiana, Mississippi, Nebraska, Nevada, North Carolina, North Dakota, Oklahoma, South Carolina, South Dakota, Tennessee, Texas, Utah, Virginia and Wyoming.[20]

When we restrict our sample to the set of counties that are both in a metropolitan area, we have relatively few cases in which one county

[15] Davis et al. (2008) find that, in 2000, about 60 percent of the variation in electricity prices paid by manufacturing plants can be explained by county fixed effects. The remaining differences may be due to multiple utilities serving a county, non-linear pricing where customers are charged both a usage fee and a peak consumption fee, or because of different rates negotiated with the utilities. Davis et al. find evidence of scale economies in delivery that are consistent with observed quantity discounts.

[16] High capital costs of nuclear plants and Public Utility Regulatory Policy Act (PURPA) contracts from the 1970s and 1980s led to substantial regional variation in retail electricity prices during the 1990s. See Joskow (1989, 2006) for a discussion of retail pricing in the electricity industry.

[17] See http://www.eia.doe.gov/cneaf/electricity/page/eia861.html

[18] In fact, industrial customers face a non-linear structure that has a per day fixed meter charge, an energy charge per kWh consumed, and an additional demand charge based on peak hourly consumption (kW) during a billing period. In addition, rates may differ by firm size and type. Some large firms face tariffs with a specific tariff that applies to them. Our empirical strategy imposes that firms respond to cross county average price variation when in fact firms will recognize that they face a non-linear pricing schedule.

[19] For counties with multiple utilities, the major utility is defined as the utility with the largest total sales across all of its industrial customers.

[20] Recently, this policy has been debated in states including New Hampshire, Missouri and Indiana. In December 2012, Michigan passed right-to-work legislation.

lies in a Right-to-Work state and the other county lies in a non-Right-to-Work State. Two examples of such a "hybrid" metropolitan areas are Kansas City, Missouri and Washington D.C. Below, we also report results in which we use all U.S. counties.[21]

3.3. Environmental regulation

The Clean Air Act assigns counties to low regulation (Attainment Status) and high regulation (Nonattainment Status) based on past ambient air pollution readings. The Environmental Protection Agency does not monitor air quality in every county. Another indicator of low regulation is if ambient air pollution is not monitored. Kahn (1997) documents higher manufacturing growth rates in counties that do not monitor ambient pollution relative to those that do monitor. Within county border-pairs, there is variation in environmental regulation both due to cross-sectional differences (i.e., high regulated counties that are adjacent to less regulated cleaner counties) and due to changes over time (reclassification of counties from attainment to nonattainment and vice-versa). In this paper, we focus on ozone as one of the six criteria pollutants. We also estimate similar models for carbon monoxide and particulate matter.[22]

We use a continuous measure of ozone pollution intensity.[23] We divide total emissions by the annual value added of each industry (from the NBER productivity data) to construct a pollution intensity index. Finally, we normalize the index to range from zero to one for ease in interpreting coefficients. We hypothesize that high-polluting industries—including petroleum and coal products, nonmetallic mineral products, and paper manufacturing—should be the most responsive to avoiding the nonattainment sides of the county border pair and in locating in that county within the county border pair that does not monitor ambient ozone. The data indicating a county's Clean Air Act regulatory status are from the EPA's Greenbook.[24] Our county/year ambient air pollution data are from the U.S. EPA AIRS data base. Our regressions include a cubic function of a county's ambient ozone level.

4. Results

Table 1 reports the summary statistics. The uneven distribution of manufacturing activity is revealed in the first row. The average county/industry/year observation has 668 jobs but the median is 111 and the maximum is 158,573. It is relevant to note that these summary statistics are based on all counties located in metropolitan areas and excludes about 75 percent of U.S. counties. Of this sample, 86 percent have at least one employee in that county, industry, and year.

Table 2 reports the names and key statistics for the 21 manufacturing industries that we study. The rows are sorted from the most energy-intensive industry (Primary Metals) to the least energy-intensive industry (Computer and Electronic Product Manufacturing). The most energy-intensive industry uses sixteen times as much electricity per unit of output as the least electricity-intensive industry. In Table 2, we report each industry's labor-to-capital ratio. Apparel, Leather, Textiles, and Furniture are some of the most labor-intensive industries. In contrast, the primary metals industry has a tiny labor-to-capital ratio. The cross-industry correlation between the electricity index and the labor-to-capital ratio equals −0.4. In the right column of Table 2, we report each of these industries' pollution intensity. Pollution intensity is positively correlated with the electricity index (0.5) and negatively correlated with the labor-to-capital ratio (−0.4).

In Table 3, we report our first estimates of Eq. (1). Recall that each county pair consists of two metropolitan area counties that are physically adjacent. Controlling for county-pair fixed effects, industry–year fixed effects, and state–year fixed effects, and a vector of county attributes (log of land area, log of the distance to the closest metro area's Central Business District, the log of the county's 1970 population, and the log of the 1990 housing values), we focus on the role of electricity prices and labor and environmental regulation in determining the geographic location of manufacturing clusters.[25] As shown in column (1), we find evidence of a negative relationship between electricity prices and manufacturing employment activity for all manufacturing industries whose normalized electricity index is greater than 0.094.[26] We find the largest negative effects of electricity prices on the most electricity-intensive industry, primary metals, has an implied price elasticity of employment of −1.65.[27]

To better understand the magnitude of these effects, assume that a state implemented a carbon price of $15 per ton of CO_2. Given the carbon intensity of producing power in different regions of the US, this can be mapped into a change in electricity prices (see Kahn and Mansur, 2010). Because of the variation in carbon-intensive electricity markets and energy-intensive manufacturing across states, our coefficients imply that the employment losses could be much larger in places like Ohio (21,884 jobs or 3.8 percent) than in California (4648, or −0.3 percent).[28]

Controlling for electricity prices, we find that labor-intensive manufacturing clusters on the Right-to-Work side of the county border pair. For the most labor-intensive industry (Apparel), the coefficients imply 443 more jobs on the right-to-work side of the border, relative to an extremely capital-intensive industry like petroleum. This is approximately half of the average number of workers in a given county/industry/year. It is relevant to contrast this finding with Holmes' (1998) work. He finds that the share of total employment that is in manufacturing is greater by about one third in Right-to-Work states. He did not disaggregate manufacturing into distinct industries. If the Right-to-Work status simply reflected this overall ideology then we might not observe that labor-intensive industries are more likely to cluster there. Our finding of a positive industry-average labor intensity interaction with the state's labor policies highlights the importance of allowing for industry disaggregation and is consistent with economic intuition.

Controlling for electricity prices and labor regulation, we also study the role of environmental regulation. As expected, we find that employment in high-pollution industries is lower in high-regulation (nonattainment) counties. We also find that employment is higher for high-ozone industries in counties that do not monitor ozone.

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[21] In metropolitan areas, there are 36 counties that make up 28 different pairs where a state line is crossed. For the full sample, 425 counties abut a state line and make 443 county pairs.

[22] We estimate Eq. (1) using two other measures of local environmental regulation intensity: a county's carbon monoxide (CO) nonattainment status; and a county's particulate matter (PM) nonattainment status.

[23] From EPA's NEI data, we aggregate total tons of emissions by industry, year and pollutant (see ftp://ftp.epa.gov/EmisInventory/2002finalnei/2002_final_v3_2007_summaries/point/allneicap_annual_11302007.zip). For ozone, we aggregate tons of nitrogen oxides and tons of volatile organic compounds.

[24] http://epa.gov/airquality/greenbk/.

[25] For the first column, when we look at the level of manufacturing employment, we use the level of population in 1970 to be consistent. The results are similar when log historic population is used instead. Recognizing that within a county, such as Los Angeles County, firms may seek out the cheapest utility within the county, we have re-estimated our models using the minimum price in the county and find very similar results.

[26] Deschenes (2012) uses a state/year panel approach using a longer time series than we do and does not disaggregate manufacturing industries beyond; "durables" and "non-durables." Controlling for state and year fixed effects, for "non-durables" he reports a positive correlation of electricity prices and employment based on a specification with state and year fixed effects.

[27] This is the sum of the coefficient on price and the coefficient on price interacted with the index (which is normalized to range from 0 to 1, where 1 is the most electricity-intensive industry (primary metals)) all divided by the average employment in our sample: $(114.6 + (-1217.6)*1)/668 = -1.65$.

[28] See Kahn and Mansur (2010) for a discussion of the assumptions regarding this application.

Table 1
Summary statistics.

Variable	Units	Obs	Mean	Std. dev.	Min	1st Quartile	Median	3rd Quartile	Max
Mnfct. employees	Workers	157,459	668	2373	0	10	111	515	158,573
% Total emp.	%	157,459	0.7%	1.8%	0.0%	0.0%	0.2%	0.7%	56.4%
ln(Employment)		135,531	4.97	2.05	0.00	3.54	5.19	6.48	11.97
Any manufacturing	0/1	157,459	0.86	0.35	0.00	1.00	1.00	1.00	1.00
Suppressed data	0/1	157,459	0.43	0.50	0.00	0.00	0.00	1.00	1.00
Electricity price	$/kWh	157,459	$0.065	$0.024	$0.000	$0.050	$0.057	$0.069	$0.523
Electricity index	kWh/shipments	157,459	0.33	0.23	0.00	0.16	0.22	0.44	1.00
Right to work laws	0/1	157,459	0.44	0.50	0.00	0.00	0.00	1.00	1.00
Labor/capital ratio	Work hours/capital	157,459	0.018	0.013	0.001	0.008	0.015	0.024	0.076
Ozone emis. rate	Tons/$MM value added	157,459	1.79	2.55	0.03	0.34	0.63	1.90	9.71
Ozone nonattainment	0/1	157,459	0.34	0.47	0.00	0.00	0.00	1.00	1.00
PM Nonattainment	0/1	157,459	0.11	0.32	0.00	0.00	0.00	0.00	1.00
CO nonattainment	0/1	157,459	0.04	0.19	0.00	0.00	0.00	0.00	1.00

Notes: An observation is by county, year, and 3-digit NAICS industry code. Index is normalized to range from zero to one.

A distinctive feature of our study is that we simultaneously study the marginal effects of energy prices, labor regulation, and environmental regulation in one unified framework. In Table 3's columns (2–4), we present our estimates for what we would find if we studied these variables individually. In column (2), we find that the electricity price interaction grows more negative by 16% and the labor intensity interaction shrinks by roughly 33% and the environmental regulation interaction grows more negative by roughly 19%.

The results in column (5) of Table 3 switch the dependent variable to the ratio of a county/year's jobs in a given industry divided by total county employment. This was Holmes' (1998) dependent variable. This measure better captures the composition of jobs within a county. The electricity price and labor regulation results are similar to the results in column (1) but in this specification we reject the hypothesis that environmental regulation is a statistically significant determinant of where manufacturing clusters. For the primary metals industry, we find that a ten percent increase in electricity prices is associated with a 0.034 percentage point reduction in the share of workers in the county who works in this industry.

In Table 3's column (6), we use the log of the county/industry/year's employment and thus lose the observations for which there are zero jobs. The electricity price and labor policy results are qualitatively quite similar to those reported in Eq. (1). Based on this specification, we estimate an employment electricity price elasticity of − 0.91 for the primary metals industry. Overall, we conclude that our environmental regulation results are sensitive to functional form assumptions.

Following Holmes (1998), the last column of Table 3 includes just small counties. Namely, the sample consists of paired counties whose centroids are within 30 miles of each other. Small counties are more likely to have similar unobserved shocks. Of course, smaller counties are likely to be in more densely populated areas as well, so we are exploring a different subset of the population. We find that the main results are qualitatively robust, with similar signs and significance, as our main findings. However, the policy effects are attenuated suggesting that there is heterogeneity in the employment effects between large and small counties. Appendix Table A2 explores how our results change across a range of centroid distances.

Given the estimates in column (1) of Table 3, we can now compare the relative sensitivities of a given industry to energy prices, labor policy, and environmental policies. For an industry like petroleum—which is energy intensive, capital intensive, and a high-ozone polluter—banning Right-to-Work laws would have the same effect on employment as an eight percent increase in electricity prices. In contrast, if a petroleum manufacturer's county falls into nonattainment with environmental regulations, this is akin to tripling electricity prices. Other industries that are not energy or pollution intensive are not as negatively affected by either higher energy prices or pollution regulation. For example, for apparel manufacturing, repealing a right-to-work law is akin to a fourfold increase in electricity prices.

In Table 4, we modify Eq. (1) by estimating separate coefficients on electricity prices for each manufacturing industry. In other words, we relax the index restriction on electricity prices that was imposed on the results reported in Table 3. We also estimate Eq. (1) separately for fifteen major non-manufacturing industries.[29] The results reported in Table 4 focus on the role of energy prices. We do not include labor or environmental regulations in these regressions. We report results for three dependent variables: the employment level, the industry's share of county employment and log employment. For ten manufacturing industries, we find negatively statistically significant correlations (at the five percent level) for the level of employment and electricity prices. For log employment, we find a negative correlation for seven of the industries. In the case of the share regressions, we find fewer negative correlations and actually find positive correlations for industries such as Textile Products (NAICS 314), Computers (NAICS 334) and Miscellaneous (NAICS 339). These two industries each have a very low energy intensity index. Finally, we note that Tables 3 and 4 imply similar employee-weighted average elasticities across industries for each specification.[30]

The bottom panel of Table 4 reports similar regressions for non-manufacturing industries. Many of these industries employ millions of people and have experienced sharp employment growth between 1998 and 2009. Employment in expanding industries such as Credit Intermediation (NAICS 522), Professional, Scientific and Technical Services (NAICS 541), and Management of Companies and Enterprises (NAICS 551) is responsive to electricity prices with elasticities of approximately − .15. However, for most non-manufacturing industries, we find that energy prices are not an important correlate of geographical concentration. An examination of BEA electricity cost shares indicates that there is not a cross-industry negative correlation between electricity prices and electricity cost shares for non-manufacturing industries.[31]

[29] We choose the 15 industries with the most employees in 1998. Wholesale electronic markets (NAICS 425) had the ninth most jobs in 1998 but the NAICS 2002 reclassifications made it difficult to track this industry. Instead, we added the 16th most common job in 1998, Motor Vehicle and Parts Dealers (NAICS 441). Note that the border-pair and state–year fixed effects differ by non-manufacturing industry but are pooled for manufacturing industries.

[30] For the linear specification, the implied elasticity is − .30 in Table 3 and − .41 in Table 4. For the log specification, they are .00 and − .10, respectively. Note that the log specification is conditional on any employment in the county/industry/year and therefore need not be the same as the linear model.

[31] We use Bureau of Economic Analysis (BEA) input–output data to construct electricity cost shares. See http://www.bea.gov/industry/io_benchmark.htm. Using data for 2002, we define the cost share as the ratio of an industry's dollars spent on electric power (NAICS 2211) over its total industry output.

Table 2
Industry details.

Industry	NAICS	Electricity	Normalized electricity		2002 Ozone emissions
		Index	Index	Labor-to-capital ratio	Rate
Primary metal manufacturing	331	0.816	1.000	0.007	2.845
Paper manufacturing	322	0.706	0.856	0.006	5.007
Textile mills	313	0.503	0.591	0.014	1.222
Nonmetallic mineral product manufacturing	327	0.454	0.527	0.013	7.046
Chemical manufacturing	325	0.402	0.459	0.004	1.897
Plastics and rubber products manufacturing	326	0.330	0.364	0.016	0.974
Wood product manufacturing	321	0.253	0.265	0.028	3.294
Petroleum and coal products manufacturing	324	0.245	0.254	0.002	9.715
Fabricated metal product manufacturing	332	0.185	0.175	0.020	0.426
Printing and related support activities	323	0.169	0.154	0.023	0.632
Textile product mills	314	0.165	0.149	0.035	0.345
Food manufacturing	311	0.149	0.128	0.013	0.749
Electrical equipment, appliance, and component manufacturing	335	0.137	0.112	0.017	0.336
Furniture and related product manufacturing	337	0.123	0.094	0.043	1.376
Leather and allied product manufacturing	316	0.110	0.077	0.035	0.547
Machinery manufacturing	333	0.103	0.068	0.014	0.156
Apparel manufacturing	315	0.102	0.067	0.047	0.028
Miscellaneous manufacturing	339	0.096	0.059	0.023	0.204
Beverage and tobacco product manufacturing	312	0.092	0.053	0.004	0.422
Transportation equipment manufacturing	336	0.086	0.045	0.011	0.401
Computer and electronic product manufacturing	334	0.051	0.000	0.007	0.038
Correlation with electricity index				−0.395	0.485
Units		kWh/shipments		Work hours/capital	Tons/$MM value added

Notes: Industries are defined by three-digit NAICS codes. Data thanks to Wayne Gray.

4.1. Additional empirical tests

In this section, we report additional regression results to test how our core results are affected by changing the sample, the sample years, including additional control variables and using different regulatory intensity measures. In Table 5's column (1), we report our results using all of the counties in the continental United States. Relative to the metro sample, the results for the full county sample yield the same coefficient signs but the absolute value of the coefficients for electricity prices and labor regulation shrinks by more than 50 percent. The coefficients on environmental regulation indicators shrink but by a much smaller percentage. In Table 5's column (2), we include linear time trends for each control variable such as population and home values to control for the possibility that counties differ with respect to their growth trajectory. The results are robust for controlling for these trends. Columns (3) and (4) use particulate matter and carbon monoxide pollution in place of the ozone for attainment status, monitoring status, high polluter industries, and concentration ratios. We find similar coefficients as in our main results but larger standard errors.[32]

We recognize that there are cases in which a county's average electricity price could be correlated with the error term. A demand side explanation argues that a boom in local employment will result in an increase in the utility's demand. This requires more expensive power plants to operate, and electricity prices will increase. Second, industrial firms have some bargaining power in negotiating rates with the electric utility. Third, imprecise measurement of a firm's electricity price: measurement error leads to an attenuation bias of OLS estimates. To address these concerns, we present instrumental variable results in Table 5's column (5). We construct instruments using the product of the local utility's capacity shares of coal, oil and gas-fired power plants and the respective annual average fuel price.[33] The sample size declines because we are missing fuel shares for some utilities. The F-Statistic for the first stage equals 1139. The key finding to emerge in this instrumental variables case is that all industries (even those with the lowest energy intensity) now have a negative employment elasticity with respect to energy prices and the effect is much larger. The other coefficients on labor and environmental regulation are consistent with our core hypotheses.

The recent deep recession has highlighted the importance of U.S. manufacturing to our economy. During a recession, few firms are creating jobs but industries and locations may differ with respect to the rate that they are shedding jobs. In Table 5's column (6), we re-estimate Eq. (1) using just two years of the data; 2008 and 2009 to see how our key explanatory variables affect employment during a major recession. The results are qualitatively similar to the full sample results reported in Table 3's column (1) but the negative effect of electricity prices on employment now holds for all industries. For the most electricity intensive industry, the implied elasticity is −1.69.[34]

An alternative strategy for studying the role of regulations and electricity prices on employment is to estimate Eq. (2) and include county fixed effects. In this case, the key interaction effects are identified from within county yearly variation in electricity prices, and the county's regulatory intensity and national changes in the industry's annual pollution intensity, labor intensity and electricity intensity. As shown in column (7), the results are remarkably similar to our results reported in Table 3's column (1) when we include border-pair fixed effects.[35]

4.2. Regulation's impact on industrial organization

The County Business Patterns data provides information for each county/industry/year on its employment count and establishment

[32] These results are not surprising given the few number of counties in nonattainment with these pollutants.

[33] The shares data are from the EIA form 860 data for 1995. The fuel prices are from the EIA: coal prices are quantity-weighted annual averages from EIA form 423; oil prices are the spot WTI; and natural gas prices are the annual Henry Hub contract 1 prices.

[34] We have also estimated this regression using data from 2007 to 2009 and find quite similar results.

[35] Incumbent firms are likely to face migration costs to relocate. If large capital costs are sunk, firms may delay relocating until their existing production facility depreciates or there are large differences in operating costs across geographic locations. One example is the Ocean Spray Corporation which plans to close its 250-worker cranberry concentrate processing plant in Bordentown, New Jersey in September 2013, and move it to Lehigh or Northampton counties in Pennsylvania. The closing facility is old and high cost. The company has claimed that it is attracted to the new Pennsylvania location because of lower power, water and trucking costs (http://www.philly.com/philly/blogs/inq-phillydeals/South-Jersey-plant-to-close-250-jobs-moved-report.html).

Table 3
Effect of regulation on manufacturing employment.

	Manufacturing employees (N)	N	N	N	Percent total employment	ln N	N (small counties)
	1	2	3	4	5	6	7
ln Electricity price	114.6	179.9			0.17*	0.25**	107.6
	(180.3)	(193.5)			(0.09)	(0.12)	(100.6)
ln Price* electricity index	− 1217.6**	− 1410.1**			− 0.51**	− 1.16***	− 570.3***
	(515.8)	(578.3)			(0.23)	(0.33)	(193.2)
Right to work* labor/capital	9430.7***		6346.8***		8.63***	9.81***	7939.2***
	(2851.9)		(2346.8)		(3.28)	(3.27)	(2201.8)
Nonattainment county	87.4*			102.1**	− 0.06**	0.02	41.0
	(46.1)			(51.2)	(0.03)	(0.03)	(37.3)
Nonattainment* pollution index	− 519.1***			− 615.1**	0.06	− 0.09	− 200.1**
	(197.7)			(245.1)	(0.09)	(0.14)	(91.9)
No pollution monitor	− 99.9**			− 99.6**	0.10***	− 0.04	− 32.8
	(44.4)			(43.8)	(0.02)	(0.04)	(41.0)
No monitor* pollution index	542.8***			550.1***	− 0.18*	0.20*	359.3***
	(110.3)			(113.7)	(0.09)	(0.10)	(89.2)
R^2	0.36	0.36	0.36	0.36	0.13	0.53	0.28
N	1,120,243	1,127,406	1,127,406	1,120,243	958,946	947,301	643,440

Notes: All regressions include cubic polynomials of ozone concentrations, county population in 1970, miles to CBD, area of county, 1990 housing values, and county–pair, industry–year, and state–year fixed effects. The omitted category is a county located in a pro-union state that does monitor air quality and is in attainment with Clean Air standards. Significance is noted at the 10% (*), 5% (**) and 1% (***) levels. Standard errors clustered by utility.

count. In Table 6, we use these two pieces of information and in addition we calculate the average employment count per establishment. We report regression estimates of Eq. (1) using each of these as the dependent variable. Table 6's column (1) is identical to Table 3's column (1). In column (2), we report the establishment count regression. We find that the count of establishments responds to both

Table 4
Employment regressions by industry.

NAICS	Employees in 1998 (1000s)	Industry growth	BEA elect. cost share	Manufacturing industries	ln N Coef.	ln N S.E.	Employees Coef.	Employees S.E.
311	1464	− .004%	1.17%	Food	0.03	(0.24)	239	(311)
312	173	− 10%	0.79%	Beverage & tobacco product	0.02	(0.41)	− 890	(396)**
313	385	− 51%	2.40%	Textile mills	− 0.31	(0.58)	− 970	(344)***
314	217	− 28%	0.77%	Textile product mills	0.17	(0.16)	− 905	(312)***
315	671	− 68%	0.54%	Apparel	0.25	(0.32)	227	(434)
316	79	− 53%	0.66%	Leather & allied product	− 0.10	(0.27)	− 1026	(380)***
321	580	− 1%	1.35%	Wood product	− 0.59	(0.23)**	− 1008	(329)***
322	568	− 22%	3.34%	Paper	− 0.47	(0.22)**	− 728	(303)**
323	845	− 24%	0.99%	Printing & related activities	0.27	(0.11)**	− 60	(119)
324	111	− 7%	0.78%	Petroleum & coal products	− 0.59	(0.25)**	− 1007	(371)***
325	901	− 11%	3.49%	Chemical	0.08	(0.19)	143	(317)
326	1030	− 13%	1.82%	Plastics & rubber products	− 0.24	(0.15)	− 240	(194)
327	508	− 5%	2.20%	Nonmetallic mineral product	− 0.33	(0.17)**	− 723	(287)**
331	615	− 27%	3.40%	Primary metal	− 1.17	(0.26)***	− 1053	(331)***
332	1816	− 14%	1.42%	Fabricated metal product	− 0.18	(0.14)	979	(555)*
333	1444	− 22%	0.47%	Machinery	− 0.31	(0.18)*	− 211	(260)
334	1681	− 37%	0.27%	Computer & electronic product	0.67	(0.26)**	2185	(910)**
335	602	− 30%	0.66%	Electrical equipment, appliance	0.11	(0.20)	− 574	(256)**
336	1911	− 15%	0.21%	Transportation equipment	− 0.80	(0.28)***	− 243	(578)
337	604	− 10%	0.70%	Furniture & related product	− 0.11	(0.14)	− 584	(155)***
339	737	− 7%	0.49%	Miscellaneous	0.71	(0.12)***	574	(194)***
				Other industries				
238	8926	26%	1.28%	Specialty trade contractors	0.10	(0.06)*	− 825	(576)
441	1757	11%	1.28%	Motor vehicle & parts dealers	− 0.06	(0.06)	− 797	(315)**
445	2944	− 1%	1.28%	Food & beverage stores	0.03	(0.12)	− 786	(412)*
452	4263	− 34%	1.28%	General merchandise stores	− 0.07	(0.06)	− 549	(288)*
522	2688	22%	0.10%	Credit intermediation & related	− 0.15	(0.08)*	− 277	(350)
524	2312	3%	0.11%	Insurance carriers & related	− 0.22	(0.12)*	− 340	(389)
541	6052	33%	0.19%	Professional, scientific & techn.	− 0.18	(0.09)*	− 4099	(1717)**
551	2704	8%	0.63%	Management of companies	− 0.15	(0.13)	− 1514	(540)***
561	8366	27%	0.28%	Administrative & support	− 0.07	(0.11)	− 3151	(1245)**
611	2324	28%	2.18%	Educational services	0.02	(0.11)	− 81	(605)
621	4482	27%	0.35%	Ambulatory health care	0.07	(0.05)	− 14	(528)
622	5011	7%	1.13%	Hospitals	− 0.13	(0.11)	463	(477)
623	2511	19%	1.38%	Nursing & residential care	0.14	(0.05)***	107	(191)
722	7758	22%	1.96%	Food services & drinking places	0.00	(0.04)	− 2854	(1218)**
813	2488	12%	0.20%	Religious, grantmaking, civic	− 0.04	(0.04)	14	(187)

Notes: For manufacturing industries, we modify Eq. (1) so that each industry has a separate price coefficient. For non-manufacturing industries, we estimate Eq. (1) separately for each industry. Industry growth is from 1998 to 2006. See Table 3's notes for further details.

Table 5
Alternative regressions exploring the relationship between regulation and manufacturing employment.

	All counties	County trends	PM regulation	CO regulation	Instrumental variables	Instrumental variables	County fixed effects
	1	2	3	4	5	6	7
ln Electricity price	67.9	3.0	23.2	− 187.7	− 3648.4**	− 469.6*	540.6***
	(55.7)	(188.9)	(193.3)	(212.7)	(1598.4)	(250.9)	(195.1)
ln Electricity price*electricity index	− 416.4**	− 1214.4**	− 1417.2***	− 1149.7***	− 2839.2	− 748.2**	− 1217.2**
	(176.3)	(515.3)	(443.6)	(387.1)	(2048.7)	(373.3)	(516.9)
Right to work*labor/capital	4416.4***	9429.0***	7750.6**	7965.4*	8302.9***	8540.1**	9432.2***
	(1209.1)	(2851.7)	(3910.6)	(4057.2)	(3060.3)	(3527.9)	(2858.7)
Nonattainment county	142.3***	113.0**	58.5	510.7**	49.6	79.6	65.6
	(36.4)	(45.3)	(115.6)	(236.4)	(55.9)	(75.2)	(41.3)
Nonattainment*pollution index	− 589.2***	− 519.5***	− 481.3	− 1150.1	− 362.4*	− 470.3**	− 519.2***
	(156.0)	(197.8)	(333.2)	(877.5)	(193.1)	(195.6)	(198.2)
No pollution monitor	− 83.5***	− 91.6**	− 122.7**	− 64.4	− 130.4**	− 81.0	− 109.3***
	(16.0)	(45.1)	(50.7)	(135.7)	(59.1)	(57.7)	(20.5)
No monitor*pollution index	330.2***	542.8***	574.3***	947.7***	602.4***	455.9***	542.8***
	(45.7)	(110.3)	(99.7)	(164.5)	(140.6)	(90.1)	(110.5)
County pair F.E.	Y	Y	Y	Y	Y	Y	
Industry–year F.E.	Y	Y	Y	Y	Y	Y	Y
State–year F.E.	Y	Y	Y	Y	Y	Y	Y
County F.E.							Y
R^2	0.37	0.37	0.34	0.34	0.43	0.39	0.38
N	3,010,812	1,120,243	1,100,173	1,104,840	798,208	185,828	182,507

Notes: Column (2) includes linear time trends for the county variables (population in 1970, miles to CBD, area of county, 1990 housing values). See Table 3's notes for further details.

electricity prices and to environmental regulation. Establishments that are energy intensive avoid the high electricity price counties. We cannot reject the hypothesis that there is no correlation between labor regulation and the establishment count. In column (3), we switch the dependent variable to the log of the establishment count. In this case, we find that there are more labor-intensive establishments clustering on the Right-to-Work side of the border. We continue to find evidence that electricity prices and ozone regulation are determinants of establishments. In columns (4) and (5) of Table 6, we report regression results for two measures of facility size: the ratio of workers per establishment, and its log. Bigger firms avoid the high electricity price county. Surprisingly, we find no statistically significant correlation between a county's Right-to-Work status and the size of facilities even for labor-intensive industries. Based on the results in column (4), smaller firms in high ozone industries are clustering in counties that do not monitor ozone.

4.3. Summary of results

We summarize our findings in Table 7. In this table, we use our regression results from Table 3's column (1) and we report our estimated effects for electricity prices, labor regulation and environmental regulation. Recall that the interaction terms between electricity prices, labor regulation and environmental regulation and the industry specific attributes listed in Table 2 play a key role in our estimates of Eq. (1). In Table 7, we exploit this information to report how the effects of electricity prices and regulation vary with industry attributes. The most intensive industries in electricity, labor and pollution are much more sensitive to their respective policies. For example, the electricity price elasticity is almost negative two for electricity intensive industries, such as primary metals, but is inelastic and only weakly significant for the average industry in our sample. Labor and environmental policies have huge effects on their most intensive

Table 6
Regulation and establishment characteristics.

	Employees	Establishments	Log establishment	Workers per establishment	Log (workers per establishment)
	1	2	3	4	5
ln Electricity price	114.6	6.3**	0.12*	10.9**	0.12**
	(180.3)	(2.8)	(0.07)	(4.4)	(0.06)
ln Price *electricity index	− 1217.6**	− 38.1**	− 0.57***	− 42.3***	− 0.59***
	(515.8)	(14.8)	(0.20)	(12.0)	(0.14)
Right to work*labor/capital	9430.7***	− 14.1	7.46***	− 117.0	2.34
	(2851.9)	(78.4)	(1.82)	(115.6)	(1.73)
Nonattainment county	87.4*	3.0***	0.05***	− 4.3***	− 0.03
	(46.1)	(1.1)	(0.02)	(1.6)	(0.02)
Nonattainment *pollution index	− 519.1***	− 17.9***	− 0.17**	12.3*	0.08
	(197.7)	(5.8)	(0.08)	(6.4)	(0.10)
No pollution monitor	− 99.9**	− 1.6	− 0.05*	4.4***	0.01
	(44.4)	(1.0)	(0.03)	(1.5)	(0.02)
No monitor *pollution index	542.8***	13.5***	0.25***	− 11.2*	− 0.06
	(110.3)	(2.9)	(0.06)	(6.4)	(0.07)
R^2	0.36	0.44	0.77	0.14	0.28
N	1,120,243	1,120,243	947,301	947,290	947,290

Notes: See Table 3's notes for details.

Table 7
Summary table of main results.

Regulation or price	Least intensive	Average intensity	Most intensive
Electricity	(e.g. Computers)		(e.g. Primary metals)
Electricity priceElasticity	0.172	− 0.227*	− 1.652***
	(0.270)	(0.134)	(0.621)
Labor	(e.g. Petroleum/coal)		(e.g. Apparel)
Right-to-workPercentage	2.1%***	21.9%***	67.0%***
	(0.6%)	(6.6%)	(20.3%)
Pollution	(e.g. Apparel)		(e.g. Petroleum/coal)
Ozone nonattainment percentage	13.1%*	4.3%	− 64.6%***
	(6.9%)	(4.7%)	(24.5%)
Ozone no monitorPercentage	− 15.0%**	− 5.8%	66.3%***
	(6.6%)	(5.9%)	(14.1%)

Notes: We report elasticities and percentages based on Table 3, Column (1) estimates and Table 1's average number of workers per observation. Standard errors are reported in parentheses using the delta method. Average intensity is a worker-weighted average of the county–industry–year observations in our sample and equals 0.28 and 0.11 for the normalized electricity and ozone indices, respectively. See Table 3's notes for further details.

industries, apparel and coal/petroleum respectively, but hardly matter for the average industry. As shown in Table 3, the electricity and labor policy findings are robust to functional form assumptions but are mixed for environmental policies.

5. Conclusion

The basic logic of cost minimization offers strong predictions concerning where different manufacturing industries will cluster across U.S. counties as a function of regulatory policies and input prices. Using a unified framework that exploits within county-pair variation in locational attributes, we have documented that labor-intensive industries locate in anti-union areas, energy-intensive industries locate in low electricity price counties and high polluting industries seek out low regulation areas. The environmental regulation finding is sensitive to functional form assumptions but previous studies have reported qualitatively similar evidence. Based on our findings, we conclude that energy prices are a significant determinant of locational choice for a handful of manufacturing industries such as primary metals. For the typical manufacturing industry, the electricity price effects are modest.

Our analysis highlights the importance of studying the marginal effects of energy regulation, labor regulation and environmental regulation at the same time. Republican "Red States" tend to have low electricity prices, and be Right to Work states while Democratic "Blue States" tend to have higher electricity prices and support union rights. Both types of states are roughly likely to have counties assigned to pollution non-attainment status. This paper's empirical strategy has allowed us to estimate the marginal and total effects of this bundle of policies.

We anticipate that future research will access census micro data for manufacturing plants. Such data would allow researchers to make more progress on the likely mechanisms underlying the aggregate effects that we report. At the extensive margin, do incumbent firms exit areas where environmental regulations tighten and electricity prices increase? Or, do existing firms respond by reducing their output and hence their consumption of inputs? Anticipating the persistence of these policies do firms make investments to alter their use of the relatively more costly input?

Appendix A. Supplementary data

Supplementary data to this article can be found online at http://dx.doi.org/10.1016/j.jpubeco.2013.03.002.

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